

## Psychological Bulletin

EDITED BY

JOHN E. ANDERSON, UNIVERSITY OF MINNESOTA

WITH THE CO-OPERATION OF

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# Psychological Bulletin

## THE STATUS OF RESEARCH IN REMINISCENCE

BY CLAUDE E. BUXTON<sup>1</sup>

*State University of Iowa*

The last general review on the topic of reminiscence was published by G. McGeoch (48) in 1935. Research since that time continues to reflect certain definitional and theoretical confusions which existed prior to her report. It is the intent of this paper to point out some of these confusions, attempt some clarification, and indicate the present status of our knowledge concerning reminiscence.

### THE DEFINITION OF REMINISCENCE

*The definitional problem.* McGeoch's definition of reminiscence made it an improvement in retention, as measured at some time after a partial degree of mastery had been attained, without formal intervenient review. The suggestion that such improvements in retention were merely the result of *informal* review is made by Williams (70) and by Woodworth (72). It has also been suggested that reminiscence is but a practice effect which results from the taking of successive tests. Brown (4) early pointed out that a first test of retention may serve to 'fix' items at or above the threshold of recall; it may also facilitate recall at the second test, in that items previously below threshold may rise above it through shifts in associative trends, etc. That items not demonstrably learned during a first practice may appear in a test after a rest interval has been shown by J. A. McGeoch (50). The definition given by G. McGeoch does not differentiate reminiscence from the gains which occur on the basis of either informal review or test-retest practice.<sup>2</sup>

<sup>1</sup> Many suggestions made by Major Arthur W. Melton have been incorporated in this review. The writer is very grateful for this critical assistance.

<sup>2</sup> Neither review nor test-retest practice fully explains why increases rather than an Ebbinghaus curve were found in the work of Ballard (1) and others. Also, as will be seen later, the temporal rise and fall of tendencies to reminiscence is difficult to reconcile with either of these "explanatory" factors.

*A revised definition.* By virtue of their experimental design, which will be discussed later, Ward (68), Hovland (32), and Melton and Stone (55) recently have made two changes in McGeoch's definition of reminiscence. First, they have attempted to prevent review of any sort, formal or informal; and second, they have ruled out practice occurring in successive retention tests on the same material. By so doing they require the demonstration of a phenomenon not explicable via the two obvious factors mentioned above. Since their design seems best fitted to serve as the basis for future work in reminiscence, a revised definition of the phenomenon, springing from their experimental operations, is indicated: *Reminiscence is an improvement in performance, as shown by some measure of ability to recall at some time after the original practice, without (any) intervening practice.* Two characteristics of this statement should be noted at once. First, an omission: no mention is made of partial mastery, although this is a widely accepted part of the definition of reminiscence. The omission is logical because degree of mastery is merely a variable in the amount of reminiscence. Second, it is specified that recall measures are the indicators of reminiscence.

*Reminiscence and relearning.* It is at this point that controversy is very likely to occur. Relearning scores have been accepted as a measure of reminiscence by many investigators (6, 32, 33, 34, 46, 47, 52, 58, 63, 68). Problems of reminiscence thus have been confused with problems in the distribution of practice, for when relearning is used as a measure of retention, the data actually arise from additional (spaced) formal practice. As a first step in the analysis of this confusion, it may be pointed out that interpolation of a filled rest interval in a learning process may plausibly have several different effects, as shown by comparison of an experimental group with a no-rest group: (a) improvement in recall plus relearning benefit (32, 54, 68); (b) improvement in recall but no relearning benefit or handicap (10, 54); (c) no improvement in recall score but relearning benefit (46); (d) no improvement in recall score and no relearning benefit or handicap (10, 46, 55); (e) loss in recall score but relearning benefit (39); (f) loss in recall score and neither handicap nor benefit in relearning; (g) loss in recall score and handicap in relearning (10, 35, 55, 68). If both recall and relearning performances are accepted as criteria of reminiscence, possibilities *a*, *b*, *c*, and *e* would be given that name, although in all but *a* the two kinds of data would be in internal conflict. The same



type of conflict would occur in attaching the label of forgetting.

Bunch and Magdsick (6), Hull *et al.* (38), and Ward (68) have indicated the possibility that changes in recall scores and changes in relearning scores may be deduced from the same general theoretical structure. At the same time it is evident from the above analysis that *empirically* these are two different sets of phenomena. *Historically* speaking, Ballard (1) and many after him (*e.g.*, 25, 48) thought it necessary to speak of reminiscence as a separate or new concept. Furthermore, phenomena of distributed practice usually have been given theoretical treatment on the basis of Jost's laws (73),<sup>3</sup> while reminiscence has in the main had its own (and different type of) theory. With empirical and traditional support, then, the present definition states that the word reminiscence should apply to *recall* data fitting the special conditions of the definition, and not to relearning data. The latter will therefore be disregarded in this review. It might be added that the construction of learning theory may be well served at present by not throwing these two great sets of problems together indiscriminately.

#### EXPERIMENTAL CONTROLS AND THE REALITY OF REMINISCENCE

##### *Verbal Learning*

*Review.* As indicated above, many investigators have been reluctant to ascribe much importance to improvements in retention after formal practice had ceased because of the possibility that the Ss had casually or intentionally revived the learned material between cessation of practice and the retention test or tests.

G. McGeoch (48) questioned school children concerning review between first and second retention tests of poetry which they had memorized. Although 84 per cent of the 3rd and 4th grade children in one of her experiments admitted reviewing in some way, retention in reviewing and non-reviewing groups (as shown by both verbatim and sense scores) was not significantly different. Forlano and Brunshwig (26) have confirmed this finding. (In neither study did reminiscence as measured in terms of group averages occur.) Unfortunately, both of these studies are subject to the test-retest error, which will be discussed below, and thus the relation of review to improvement in retention by individual Ss is not adequately indicated by either of them. Also, there is room for doubt as to the validity of answers to questions about whether review occurred during the 24 hr. intervals they employed.

<sup>3</sup> Youtz (73) points out (p. 4) that the phrase "a new repetition" which Jost used has customarily been expanded to mean "further repetitions." Her work shows that this expansion is justified, and it is this form of Jost's law which is here related to relearning.

Because of the ambiguities involved in the evaluation of apparent reminiscence where review is possible, and because of the lack of convincing proof that casual review does not enhance retention, it seems necessary that this factor be eliminated before a valid finding of reminiscence can be claimed. The definition of the phenomenon given above reflects this conclusion, and many studies, on this basis, are open to criticism (1, 14, 18, 19, 20, 22, 23, 25, 29, 45, 53, 56, 58).

There are three ways in which to treat review as a factor in retention. The first is to require *S* to do something during the rest interval which should preclude review.

This technique is exemplified by studies which have required *S* to spend the interval in light reading (10, 63, 68), color naming or color association (15, 32, 55, 68), arithmetic problems or number cancellation (15, 51), or motor or mechanical activities (12, 13, 46, 63). The use of such activities during the rest interval or, indeed, the use of an "unfilled" rest period, makes the reminiscence experiment related to the retroactive inhibition experiment, as well as to those on work decrement (2, 62) or mental set and shift (40).

More specifically *E* must, *in the reminiscence experiment*, recognize that *S*'s retention may be affected by shifting attention from one task to another quickly, or being set to make such shifts.<sup>4</sup> There is also the question whether gains during the rest interval are simply signs of a recovery from fatigue, i.e., whether they are due to performance rather than to learning or memory factors.

Recognition of such relationships is made necessary also by the to-be-expected finding that the interpolated activities thus far employed have different effects upon retention. Ward (68) indicates, for example, that color association permitted more reminiscence to appear than light reading, and he suggests that color association might have a disinhibiting effect, in the Pavlovian sense. Unfortunately, the effect of color association was studied in a supplementary experiment with only 6 *Ss*. There is thus an additional interpretation of his finding: the light reading had a slight retroactive effect (which is particularly possible in any reminiscence experiment employing verbal learning). Shipley (63), like Ward,

<sup>4</sup> A possibility needing exploration is that reminiscence may not be found in experiments employing interpolated activities (e.g. color naming) unless *S* is well-practiced at shifting from learning to naming and back again. That is, at the time of recall *S* may not have the appropriate set unless he is accustomed to making the rapid shifts required in such experiments, and his recall score will suffer accordingly.

found that two types of rest-interval activity were not equal in their effects upon retention: Ss who worked at a mechanical toy retained slightly more than Ss who read *College Humor*. Reminiscence as shown by means did not appear in this research, but the interpretation of the effects of interpolation is the same. It is apparent, on the basis of experiments on retroactive inhibition, that the activity which fills a "rest" interval in a reminiscence experiment should be dissimilar to the learning activity.

A further consideration in experiments employing the interval-filling technique is that although they may restrict review greatly, there is as yet no guaranteed method of eliminating it. For example, in Hovland's well-controlled experiments (32, 33, 34) there is still a possibility that Ss could review while they were naming colors at a 2 sec. rate. This also might be said of number-cancellation in the experiments of McGeoch, McKinney and Peters (51). The rapid color-naming used by Melton and Stone (55) seems to be an improvement over the procedure of Hovland or Ward because it keeps S busier. On the other hand, the light reading employed by several investigators is suspect for there is little or no objective indication as to whether S is reading or simply turning the pages while he thinks about what he has learned. Filling the rest interval with mechanical or motor activities might distract S very little from review. Finally, in experiments which reduce individual differences in memorization by using the same Ss for many sittings with different lists, there is the possibility that as S becomes more and more aware of what is expected of him he will become more skillful at reviewing on the sly. This possibility can not be checked in data of experiments done to date because an increase in apparent reminiscence would be confounded with variance due to practice at the original learning and recall techniques, as well as with variance due to lists, which have been used in the same order for all Ss. It is subject to both objective and introspective checks, however.

The second treatment of the factor of review is to use questionnaire or other methods to determine how much reviewing there has been, and to discard Ss who admit it, or to analyze their data separately. In all the reminiscence experiments in which review has been prevented or restricted, rest intervals were 20 min. or less in length. Since it is unlikely that any procedure will soon be hit upon to rule out review, where the interval is too long for E to control S's activities, it seems necessary that any study involving such intervals should as a minimum follow the procedure of Mc-

Geoch or Forlano and Brunschwig (see above) and attempt to determine how much reviewing there has been. Further use and investigation of the procedure of requesting Ss not to review, as was done by Hovland (35), Raffel (59) and Ward (68), seems worthwhile. Systematic studies of this sort may lead to some correction of retention curves over the longer intervals.

The third treatment of the factor of review is fairly well demonstrated by the study of Raffel (59), in which Ss were *forced* to review via recall tests given each day or at longer intervals. At each test she was able to note the occurrence of repeated or previously unrecalled items. Raffel's procedure and variations of it should, like the second treatment suggested above, permit evaluation of the effects of review rather than elimination of it.

*Test-retest practice effects.* As pointed out previously, and as implied by the definition of reminiscence, if retention is measured by two or more tests over the same material with the same Ss, an improvement in retention can not be called reminiscence because the first test serves as an additional practice period.

Three authors have recently reported special studies of this factor. Bunch (5) had students in general psychology study poetry. He then asked for a written reproduction, followed immediately by a second reproduction. There was a statistically reliable increase in the average number of lines correctly recalled, and 61 per cent of the Ss showed improvement. Gray (29) studied retention of word lists, sentences and paragraphs, where a delayed recall preceded by an immediate could be compared with a delayed recall *not* preceded by an immediate. The double-test groups all showed statistically reliable superiority (but no reminiscence). The application of such findings to reminiscence experiments in general is clear, but special attention is merited by an experiment of Deese (15). Ss studied poetry, then were given rest intervals of 0, 2, 5, and 20 min. Review was precluded or restricted by requiring color-naming and other activities of S; test-retest practice effects were ruled out by the nature of the testing program. Under these conditions *no* reminiscence, in terms of averages, was found.<sup>5</sup> In fact, there was a definite loss by 20 min., and no reason to believe that with a longer interval retention would have improved again. This finding throws considerable suspicion on the many experiments where these controls were not applied (14, 15, 22, 23, 24, 45, 51, 53, 56).

Two simple experimental designs enable *E* to either rule out test-retest practice effects or hold them constant during the study of other factors in reminiscence. One of these designs (5, 29) in-

<sup>5</sup> Other measures, *e.g.*, the per cent of Ss showing improvement, might have shown the existence of reminiscence, but its statistical reliability presumably would not be great.

volves (a) bringing an experimental and a control group to equivalent levels in their learning, (b) giving the control group an immediate test, (c) giving the experimental group its first test after some time interval. The difference between the two tests, one for each group, will give an indication of the amount of reminiscence which can be evaluated statistically. Where *E* has no record of the progress of learning and must work with the single recall which follows learning, the initial matching of groups may be difficult. This is not the case in serial learning. *E* here has a continuous record of the level of mastery and can equate groups by specifying a criterion which is to be reached before a rest interval is given. Ward (68), for example, used a control condition in which *S* learned to one perfect trial and without rest had another trial (immediate recall test), and an experimental condition in which *S* rested between the first perfect trial and the next trial (delayed recall).<sup>6</sup> Hovland (32, 33, 34), McClelland (46), Melton and Stone (55), and Shipley (63) have also utilized this procedure.

Another design (5) employs an immediate test for both a control and an experimental group. A second test occurs at once for the control group, and after any desired time interval for the experimental. The difference between second tests for the two groups is the measure of reminiscence. This method aids in the matching of experimental and control groups where the type of learning procedure makes this matching a problem (see above), but it holds test-retest practice constant rather than eliminating it.

Either of the two designs (and it is not implied that these are the only two) has its advantages, but the point which should be stressed is that the adoption of a fairly standard design would aid greatly in improving the quality and comparability of various researches in reminiscence. The designs of Ward, Hovland, and Melton and Stone treat test-retest practice in the same way (elim-

<sup>6</sup> In his supplementary experiment A, Ward (68) determined the average number of trials for each of 6 *Ss* to reach a criterion of 7 out of 12 items. He then used the same *Ss* in supplementary experiment B. Here, to equate the original learning in control and experimental conditions, each *S* was given that average number of trials, rather than being carried to the criterion itself. Since the original learning was not stopped when an arbitrary criterion was reached, *Ss* should drop but little on their recall in the control condition. Should recall in the experimental condition be reliably superior this would be evidence that reminiscence in serial learning is independent of the use of an arbitrary mastery criterion. Ward's results point in this direction, but the *N* was small, reliability evaluations were not made, and the *Ss* were sophisticated.



nating it) and may serve as a model for future work in this respect. At the same time, even they have not standardized the method to be used in filling the rest interval, and this problem requires study.

### *Motor Learning*

Although reminiscence in motor learning has not received a great deal of attention, this is an area in which studies of the distribution of practice have been very numerous, and therefore is an area in which the extent of occurrence of reminiscence greatly needs evaluation. To permit consistent usage of a definition of reminiscence in terms of a recall measure, it is necessary to assume or design the experiment so that skill in motor learning, as shown by a trial score, is analogous to, *e.g.*, the number of items correct on a recall trial in serial learning.

*Review.* In contrast to verbal learning, practice of a motor skill during the rest interval is preventable insofar as overt practice is preventable. In pursuit learning, for example, *E* has only to keep *S* away from the instrument, or to prevent other overt movements which might transfer to the pursuit task. Doré and Hilgard (16, 17) have published data showing evidence of reminiscence in this task, and Snoddy (65) in mirror tracing. Covert practice during the rest interval, while probably less effective with these tasks than review with verbal materials, is nevertheless a real possibility. Bray (3), for example, showed that symbolic practice given via coaching transferred to mirror target performance; Buxton (7) has indicated some of the possible ideational factors in pursuit learning, although direct studies of them are few in number (60). The suggestions made concerning elimination or control of review in verbal learning experiments are all three capable of application to motor learning experiments.

*Test-retest practice effects.* The first experimental design for eliminating such practice effects, *viz.*, bringing control and experimental groups to the same level of learning, giving the control group an immediate test and the experimental a later test, is particularly applicable to motor learning studies. As in serial learning, most motor learning tasks are so devised that there is evidence of the level of learning in performance on each trial. Specific mastery criteria may be used to equate groups, as in anticipation learning.

### *Summary*

The question may arise: is there *any* valid evidence of reminiscence? Large numbers of studies are invalidated because they fail

to exercise one or the other of the controls discussed above. It seems advisable to require that *all* proper controls should be exercised before evidence is accepted. In this light, the basic experiment of Ward (68) can not be accepted completely, because of the possible influence of review during the interval spent in light reading, and because his supplementary experiment which required color association was not thorough enough. Well-controlled experiments in which reminiscence failed to appear are those of Deese (15) and Melton and Stone (55), but as we shall see later the failure probably was due not so much to the control of test-retest practice and of review, but to other and legitimate experimental variables. The experiments on motor learning need checking under controlled conditions in which a direct attempt is made to study reminiscence. Hovland's systematic experiments (32, 33, 34) remain as the most substantial evidence for the reality of reminiscence—and even here, as suggested previously, there is the haunting possibility of review.

#### MEASURES OF REMINISCENCE

*Type of data collected.* Various indicators of retention, based on the recall trial, have been employed. Nearly all of the better-controlled experiments described in this review recorded the number of items correctly anticipated in serial verbal learning (32, 33, 34, 55, 68). There are few equally good data based on other types of recall which may be used for purposes of comparison.

For example, Hovland (34), working with paired associate material and performing well-controlled experiments, employed the method of right associates in the recall test and found little or no reminiscence, but, as will be discussed later, the method as such was probably not the reason. McGeoch, McKinney and Peters (51), however, also used paired associate material (in experiments open to the test-retest error) and found reminiscence.<sup>7</sup> Written reproductions likewise have often been required, but of five experiments, all open to test-retest and review errors, three (14, 45, 48) found reminiscence and two (26, 56) did not. An association test was used by Raffel (59) to measure retention of a list of 100 familiar words; *S* combined free association and oral reproduction in an attempt to say 150 words, credit being given for correct words from the list. Witmer (71) required *Ss* who had learned a list by the anticipation method to respond, in the retention test, by saying the first syllable which came to mind when *E* presented the items of the original list in mixed order. There was some evidence of reminiscence in both these studies but both are open to criticism.

<sup>7</sup> The comparison is complicated by several other differences in procedure. See Hovland (34, pp. 477-479) for a discussion of these points.

It may be said that this paragraph exemplifies the lack of comparative data on measurement procedures in this field.

Simley (64) showed that items which were slow to rise above the threshold of recall in paired associate learning had longer reaction latencies than items which rose more quickly above the threshold. Some shortening of the latencies occurred thereafter in either case, which Simley interpreted as evidence that the associations were increasing in strength.<sup>8</sup> Following Simley's interpretation, measures of response latencies during the recall trial have been employed as indicators of retention by Ward (68), Melton and Stone (55), and McClelland (46). This technique gives a more finely scaled indication of the level of retention than the simple recording of presence or absence of response, and the measurements are sufficiently precise to reveal progressive latency decrements as amount of practice on adjective lists increases (55). However, the latency measurements may be of low reliability, particularly for central items in a list. This possibility, as pointed out by Melton and Stone (55, footnote 6), is difficult or impossible to check because "... the same subjects do not yield latency measurements for a particular item under all experimental conditions, or ... the number of latency measurements for a particular trial varies from subject to subject and from condition to condition." Reaction-latency measures thus seem to be of doubtful value, or at least are difficult to interpret.

Performance scores on a recall trial may be used in studies of reminiscence in skill acquisition (8). Snoddy (65), in mirror tracing, for example, used velocity scores based on the pacing of *S* to equate the number of errors and the seconds necessary for a circuit. Renshaw and Schwarzbek (59) used errors per cycle on a pursuit meter; Doré and Hilgard (16, 17) used length-of-contact scores on a pursuit rotor, as did Travis (66, 67) on this apparatus and the pursuit oscillator. The amount of letter-code translation per trial was the measure of learning in the work of Gentry (27) and Lorge (44).

The principal conclusion to be drawn from the above discussion is that at present there is no indication that the occurrence of reminiscence depends upon the type of recall or recall measure. The conclusion is drawn in the absence of comparative studies rather than on the basis of such studies.

<sup>8</sup> Reaction latencies tend to become asymptotic. Simley suggests, however, that strengthening of the *association* may continue *after* this happens.

*Treatments of data.* Most investigators in the field of reminiscence have reported measures (e.g., means) based on the performance of a total group of Ss. G. McGeoch (48) argued that such measures are not valid in the study of reminiscence, for reminiscence is an affair of the memory of an individual S, who either improves or does not. Averages can conceal the fact that many Ss have improved slightly while one happens to lose greatly in retention score. Following Ballard (1), she employed a supplementary measure which is quite widely used—the proportion of Ss who show improvement in individual total score (20, 26, 32, 33, 34, 45, 51, 59). Several of these investigators also report the proportion of Ss whose score does not change from the initial to the final test, and the proportion whose score decreases. In some instances, the average change in a total sub-group of Ss whose scores improve or become poorer is reported as a supplement to the proportion of Ss in the sub-group itself (1, 48, 51).

Underlying the use of the percentage of Ss showing improvement in retention, as evidence of reminiscence, is the notion that there are individual differences in the display of this phenomenon—some Ss are reminiscers, some are not. There is as yet no study which reliably demonstrates this,<sup>9</sup> nor are there direct studies of the degree to which the percentage of Ss who improve varies with experimental conditions.

There is a more important consideration of the use of this measure to date: the mere fact that a certain number or percentage of Ss improves in retention does not prove that reminiscence is occurring. There are three possible causes of such a finding: (a) In many studies the test-retest error is involved. One might expect that on a chance basis some Ss would profit sufficiently from the first test to compensate for their forgetting. (b) At the time of the first test, certain items are likely to be very near (above or below) the threshold of recall. By chance more items might rise above the threshold than dropped below it for certain Ss and improvement in total retention score would be noted for these Ss.<sup>10</sup> (c) In studies

<sup>9</sup> The closest approach is found in a study by McGeoch, McKinney, and Peters (51), where there are indications that the *amount* of improvement in Ss showing improvement might be a constant under different experimental conditions.

<sup>10</sup> The reaction threshold, a concept employed by Simley (64), is developed by Hull *et al.* (38). The level of this threshold for a given item is assumed to vary from trial to trial, according to a normal probability curve (postulate 15). It thus is possible for an item to be anticipated correctly, then be missed for one or more trials before it re-appears, or never to be anticipated correctly until the final recall.

employing systematically counterbalanced control and experimental conditions (30, 66), some of the Ss might be expected to show retention increases because of the chance location of the control condition at an unfavorable place in the practice series. Therefore the number or percentage of Ss showing improvement, standing alone, is not an adequate indicator of the amount of reminiscence. After the test-retest error is ruled out, two further treatments are required: (a) the reliability of such measures must be determined; (b) they must be compared statistically for the control (no rest) and the experimental conditions. Only when the number or percentage of Ss improving is used in this way will it approximate the validity of means as a measure of reminiscence (assuming that the latter are given the same rigorous tests).

A more detailed treatment of data has been used by a few investigators. This is the computation of the proportion of new and/or old and/or lost items. In a sense this type of treatment carries the argument against the use of group-total retention data a step further: reminiscence is an affair of individual items, not an affair of total score (24).

As examples of the procedure, Luh and Liang (45) used the percentage of reminisced and forgotten items in comparing the immediate and the delayed test; Martin (53) recorded for each *S* the number of items available to reminiscence, *i.e.*, not given on the first recall, and thereafter the number of items reminisced and not reminisced out of this available number; Raffel (59) reports the mean number of words reported for the first time on each recall, as well as the number repeated; Edwards and English (19, 23) record the proportions of items which fall in various *patterns* of right and wrong on two or three successive tests (all of these studies were open to the test-retest error and review).

The statistical checks necessary for treatment of percentages of Ss showing improvement are equally necessary for the item measures. This point should be clearly understood, for reminiscence theory must be concerned first of all with individual items, and only secondarily with other measures.

#### FACTORS IN THE AMOUNT OF REMINISCENCE DISPLAYED

G. McGeoch (48) indicated that up to 1935 there was no reliable evidence that the occurrence of reminiscence was related to the age, sex, or intelligence of the learner. This conclusion still holds, but there has been little or no further work directed specifically at these characteristics of the learner and so they will not be discussed here.



*Learning technique.* In the research of Ward (68) and Hovland (32) and the theories of Hull *et al.* (38) the existence of intra-serial interference or inhibition during the original learning is deemed the important condition for the occurrence of reminiscence after a rest interval. It thus becomes important to know whether the type of learning technique employed (anticipation, serial learning) is a determiner of the presence of such interference, and therefore of reminiscence. One list is said to exhibit more of such interference than another if, (a) on the first, *S* requires more trials to reach a given mastery criterion, (b) the relative difficulty of center items, as compared to end positions, is greater in the first, and/or (c) oscillations about the threshold of recall are of wider extent in the first, as measured by the difference between the number of trials preceding the one on which a given item first is anticipated correctly and the number of the last trial on which it is missed. With a 2 sec. rate of exposure, Hovland found all three signs of intra-serial interference more clearly indicated than with a 4 sec. rate, and reminiscence occurred with the former but not with the latter.

Hovland's instructions required that if *S* made an error or failed to respond within the anticipation interval he had to correct himself *and also* attempt to anticipate, during the next such interval. In contrast, Ward (68) and Melton and Stone (55) did not instruct their *Ss* to make such corrections. It seems very possible that if corrections are required they may be an added source of confusion, *i.e.*, intra-serial interference. Furthermore, Ward required the spelling of nonsense syllables, whereas Hovland required pronunciation, as did Melton and Stone with adjectives. If spelling is also deemed a source of intra-serial interference, because it is more complicated than pronunciation, the following speculation might be made: Ward found reminiscence partly because he used the spelling procedure; Hovland found it partly because he used the correction procedure; Melton and Stone did not find it because they employed neither subsidiary procedure. Even within serial anticipation learning, variations in technique may be significant for the occurrence of reminiscence.

There seems to be only one attempt to study the effect of two clearly different learning techniques with the same type of material. Hovland (34) compared retention of serial lists and paired-associate lists of nonsense syllables (order of pairs randomized), the anticipation method being employed with each. Reminiscence was present in the former but absent in the latter except in terms of a minority of *Ss* who showed improvement. In analyzing this finding Hovland points out (p. 481) that the 2 sec. rate was used with the serial learning but in the paired-associate learning 2 secs.

were allowed for the exposure of the stimulus item of a pair (*i.e.*, for anticipation) and 2 secs. for exposure of the response item (and for corrections). In effect, the two learning techniques were not comparable because responses were 2 secs. apart at a maximum in the serial learning and 4 secs. apart in the paired-associates. The experiment is not conclusive, then, concerning differences in technique or opportunities for interference to occur, unless it be admitted that the response-rate differences are inherent in the procedure. Hovland suggests that this may be the case (p. 481).

Reminiscence has been found with several variations of the method of complete exposure, but the data which might permit comparison with the previously mentioned learning methods are affected by test-retest practice (14, 19, 20, 22, 23, 25, 45, 51, 59).<sup>11</sup> What might be called an incidental method of learning was employed in Martin's study (53): *S* performed many simple tasks and later was asked to name them. Conventional practice trials have, of course, been the learning method in the studies of skill acquisition which seem to indicate reminiscence (8).

If we accept interference as an important variable in reminiscence, we can assume that the various techniques mentioned will determine whether the phenomenon occurs insofar as they, by their nature (type of response required, degree of pace-forcing, etc.) produce interference. As yet we have only the serial learning variations to support this statement.

*Subject matter.* To return to the fact that Hovland (32) found reminiscence with syllables, while Melton and Stone (55) with adjectives did not, it may be noted that the latter investigators used both a 2 sec. rate and a 1.45 sec. rate, and showed that in terms of all three criteria listed above, interference increased as the rate of exposure was stepped up. However, the increase in number of trials to criterion and the increase in relative difficulty in center items were both unreliable, and a statistical comparison of oscillations about the threshold of recall was not made. (There was, nevertheless, a consistent trend in all comparisons.) The question can be raised: even if the increase in interference *does* occur in adjectives if exposure-rate is stepped up, is the actual amount of interference large enough to serve as the basis of reminiscence? Syllable and adjective lists must be compared directly to answer

<sup>11</sup> The above studies employed the whole method of learning. Since G. McGeoch (48) showed that there was not a significant difference in amount of reminiscence when the part and whole methods were employed, this topic has not been investigated. Repetition of her work, eliminating the test-retest practice and including the checks on review, is needed.

this question. At present the contrast between Hovland's findings and those of Melton and Stone shows that the relation of Hovland's three indicators of interference to the occurrence of reminiscence has yet to be clearly established. This is shown in another way by McClelland's study (46) of discrimination learning with adjective pairs, in which greater difficulty was evidenced with the faster of two exposure rates, but there was only a very slightly greater (and unreliable) tendency for reminiscence to appear with the faster rate.

A dozen or more different types of learning materials have been used in various studies of reminiscence. Because almost all the experiments themselves are open to criticism on the basis of design, no attempt will be made here to catalog the materials. The principal dimension in which they differ is the amount of meaning or organization which they possess for *S*, varying from nonsense-syllable lists to ballad poetry. It seems very possible that intramaterial interference might be a widespread phenomenon, although measurement of such interference is likely to be more difficult in, say, a prose passage, than in serial learning. If we grant the importance of such interference, and speculate that it might decrease as the degree of organization or meaning increases, we should expect to find that type of subject matter is a determiner of reminiscence. That this is the case is clearly suggested only by the findings of Melton and Stone.

Special attention should be paid to the apparent dependence of reminiscence upon what is called the type of learning by English and his associates (18, 19, 20, 22, 23, 24, 25). In their work, *Ss* studied a prose passage; retention was measured by a true-false test containing two kinds of items: summary (*S*, covering the content of a sentence, or several sentences, or even a paragraph) and verbatim (*V*, based on some exact statement from the text). Their Table 5 in (23) and Fig. 2 in (18) apparently show that there is improvement when retention is measured by *S* items and loss when it is measured by *V* items. The work can be criticized in several ways: (a) It was open to test-retest and review errors—which would not explain the differential retention curves. (b) Several of the experiments were based on the learning of a prose passage of psychological nature; the *Ss* were psychology students and it might be expected that transfer would benefit the *S* items much more than the *V* items, and thus produce differential retention. (c) The published data all deal with *absolute* numbers of reminiscent or forgotten items. This completely disregards the fact that, since *S* items were more difficult and more often wrong on the first test, the opportunity for reminiscence in them was much greater than in *V* items. The latter were easier and more often right on the first test, and consequently offered more opportunity for forgetting.

Buxton (9) applied chi-square tests to some of the published data and was able to show that in at least one of the studies (23) the occurrence of reminiscence (and also of forgetting) was independent of type of item when opportunity for the occurrence of reminiscence (or forgetting) was taken into account in the analysis. Also, the occurrence or non-occurrence of reminiscence was independent of difficulty, while the occurrence of forgetting *did* depend upon difficulty of items. Bearing in mind the necessity for the elimination of test-retest practice and review, and for the control of transfer and difficulty, and finally for the use of relative data (which will not be necessary when difficulty is controlled), the conclusion must be that at present the studies of English and his colleagues do not constitute valid evidence that type of learning (or, more correctly, the type of measurement of retention of a given subject matter) is related to amount of reminiscence. These criticisms have been discussed by English (21).

Unfortunately, method of learning and type of subject matter have tended to vary together. Most of the work with nonsense syllables has been done with the anticipation method, much of the work with poetry has been with complete exposure and written reproduction, and much of the work with motor tasks has been done in conventional practice trials. To a certain extent, this is unavoidable. But also, to a certain extent, ingenuity can be used in applying a given method to various subject matters, or various methods to the same subject matter. In any attempt to secure such comparative data, the equating of levels of mastery, or amounts of practice, difficulty of items, etc., requires special attention.

*Degree of mastery before rest.* Ward (68) compared amounts of reminiscence in 12-item syllable lists learned to a criterion of 7 correct with amounts in lists learned to one perfect recitation. Because serial position as well as the number of repetitions, governs the degree of learning, he made an analysis (discussed below) in which he noted amounts of reminiscence in central syllables and amounts in end syllables separately, concluding that "... the results at the shorter intervals of 5 minutes or less indicate that relative retention and amount of reminiscence are inversely related to degree of learning" (p. 39).

Certain data from two of Hovland's papers (32, Tables 1, 2, 4; 33, Tables 1, 2, 3) serve to extend Ward's conclusions somewhat. Hovland's evidence indicates that reminiscence is not as great in absolute amount after a low degree of mastery (1 trial only), or a high degree (1 perfect trial), as it is after a moderate degree of mastery (7 out of 12 correct). To generalize, one might expect the

following: with a low degree of mastery few items have risen near enough to the threshold, at the time of the rest interval, to be available for reminiscence; with a high degree of mastery few items are not already above threshold. Both stages of mastery should be inferior in *absolute* amount of reminiscence (number of items reminisced, without regard to number in list) to moderate mastery. The *relative* amount of reminiscence (per cent which the number of reminisced items is of the number in the whole list) might, on the other hand, grow progressively smaller as initial level of learning rises (8). Findings with a pursuit skill support this expectation (11).

*Serial position.* Ward (68, p. 38) compared amounts of reminiscence for items in positions 3-4-11-12 in a 12-item list with amounts for items in positions 6-7-8-9. Whether the list had been learned to one perfect recitation or to 7 out of 12 correct before the rest interval, items in the center of the list showed more reminiscence. This finding is supported by the work of Hovland (32, 33). Ward felt that in his experiment degree of learning could not be separated from serial position, and hence amount of reminiscence could not be related to serial position as such. Hovland (32), however, brought lists to about the same degree of learning by spaced and massed practice and thus was able to state (p. 218):

The syllables in the central portions of the lists learned by massed practice represented, in terms of the criterion of recall score, the same degree of learning as did those in the lists learned by distributed practice. One series, however, (massed) showed much more reminiscence than the other. This type of evidence tends to suggest that degree of learning, *per se*, is not the fundamental factor determining reminiscence, although it is probably closely related to it.

This conclusion qualifies the generalization made concerning degree of mastery and amount of reminiscence by the present writer, for Hovland's finding emphasizes the fact that at any degree of mastery the experimental conditions may be such as to produce reminiscence or fail to produce it. On the other hand, the fact that an item is in a central or end position is supposed to be a determiner of the relative strengths of excitatory and inhibitory tendencies affecting it, so that serial position as such seems to be an indirect factor in the occurrence of reminiscence.

It may be well to examine a little more closely Hovland's statement that the massed and distributed practice conditions brought about the same degree of learning in his experiment. In his Table



5 (32, p. 217), are shown mean numbers of errors in the various syllable positions in immediate recall after learning to a criterion of 7 out of 12 syllables correct by both kinds of practice. For massed practice, the mean number of errors in positions 3-4-11-12 was .254, and in positions 6-7-8-9, .684. For distributed practice, the mean numbers of errors in the same positions were .246 and .551. Therefore the list learned by distributed practice was actually learned better (statistical evaluation of the superiority was not carried out), and although less reminiscence occurred in it, there still is a trace of the troublesome co-variable, degree of mastery before the rest interval.

*Rate of exposure of items.* Hovland (33) shows that there is little or no reminiscence (in mean recall scores) with serial anticipation learning of nonsense syllables at a 4 sec. rate, although at the 2 sec. rate, as mentioned previously, there is equally clear evidence that reminiscence does occur. The rate of exposure thus seems to be a significant variable in the determination of reminiscence. Melton and Stone (55), however, finding no reminiscence when they used a 2 sec. rate with adjective lists, stepped the rate up to 1.45 secs. Although the increase in rate of exposure tended to have three effects on the original learning thought essential by Hovland and Hull *et al.* (38) for reminiscence to occur, it still failed to appear. As suggested previously, it is possible that Melton and Stone employed a kind of subject matter where reminiscence should not occur under any circumstances—making a change in rate of exposure non-critical—or the difference between their results and those of Hovland may lie in the absence of the correction procedure on the part of *S*. Although there is thus some lack of clarity in the comparison of these two studies, it seems likely that where reminiscence occurs it will be greater in amount with the faster rates of exposure. The results of McClelland (46) and Shipley (63), although they show no reminiscence, are in line with this generalization.

Deese (16) points out that seemingly one of the essential conditions for the appearance of the phenomenon is the forcing of a rapid learning rate upon *S*. The necessity for the rapid rate may explain why Hovland (34) did not find reminiscence with his particular paired-associates anticipation technique, and why Deese found no reminiscence when her *Ss* learned poetry in a relatively free set of circumstances (method of complete exposure), but it does not explain the Melton and Stone (55) results.

The rate of acquisition itself, as a variable in reminiscence, has not been studied. The data on degree of mastery, and inspection of certain results obtained by the reviewer (10), suggest that where the original learning is proceeding rapidly as mastery is approached the immediate recall is likely to be at a high level. The difficulty of the material learned (or perhaps differences in "learning ability") thus is a variable in reminiscence, for a high score in the control condition makes comparative improvement impossible after a rest interval. Conversely, where learning proceeds slowly to any mastery criterion, the immediate recall is likely to be poorer, and reminiscence can appear. The differences between syllable and adjective lists, and between long and short lists might be interpreted thus.

*Amount of material to be learned.* Although there are no well-controlled studies of reminiscence in which the amount of material to be learned was varied systematically, Hull *et al.* (38) have deduced (theorems 51-53 inclusive, plus corollaries) that the longer the list the greater the amount of reminiscence to be expected, as measured by both the number of items rising above the threshold of recall and the shortening of reaction latencies. Shipley (63), although he found no reminiscence, was able to show that retention over a rest interval was better the longer the syllable list memorized.

If we assume that distributed practice prevents the rapid accumulation of inhibitory tendencies, the disappearance of which during rest is basic to the occurrence of reminiscence, a study by Hovland (36) becomes pertinent here. He shows (Fig. 2, p. 275) that the mean number of failures to mastery at various syllable positions in a list is greater for massed practice than for distributed, and that the discrepancy becomes greater as the compared lists increase from 8 through 11 to 14 units in length. If the number of failures to mastery in massed practice is thought of as part of the picture of greater difficulty and also the greater number of re-enforcements necessary in learning by this method, it is evident that the amount of internal inhibition generated in the learning of longer lists by massed practice must be greater than for shorter ones.

Thus the prediction concerning the absolute number of items reminisced seems plausible, but there is no basis for a prediction that the *relative* amount of reminiscence will also increase; nor is there a basis for predictions concerning reminiscence as related to amounts of materials other than syllable lists.

*Type of practice.* Hovland (32) shows clearly that reminiscence is much more likely to occur if the learning is by massed rather than spaced practice. This is another demonstration of the importance of "forcing," although, as shown by Melton and Stone (55), massed practice even at a high exposure rate is not sufficient to guarantee that reminiscence will occur.

*Length of rest interval between original learning and test.* The maximum amount of reminiscence in nonsense materials apparently occurs from 30 sec. to 2 min. after the original learning (35, 68). By about 10 min. retention is slightly poorer than immediately after learning, and by 20 min. it is definitely less. It is probable that the "optimum" interval depends upon a good many factors, e.g., length of list (38, theorem 52 and corollaries 1 and 2, theorem 53) and type of material learned, but no studies have been done which permit definitive statements about its length. On the other hand, Deese (16) believes that it is likely to be relatively short.

The finding of reminiscence in mirror-tracing (65) at an interval of 24 hrs. is about the only clear suggestion that the phenomenon may occur over intervals longer than about 5 min. The drawback of Snoddy's study at present is the possibility that his Ss rehearsed symbolically during the 24 hr. interval, but motor learning (including the pursuit tasks) seems quite likely at least to have a different optimum interval than verbal learning. Many studies apparently indicate that reminiscence can occur over longer intervals (14, 19, 25, 45), including intervals up to 90 days in the work of English and his associates. All of these studies are open to criticism on various grounds, so that it is not justifiable to conclude that they demonstrate effectiveness of rest intervals of any length.

#### THEORIES OF REMINISCENCE

Hovland (32) and other writers have reviewed several theories of reminiscence and discarded them as unsatisfactory. "Explanations" of the phenomenon have been based, for example, on the principle of perseveration, motivational changes, neural growth, recovery from fatigue, and recovery from refractory phase decrement. Apart from the lack of specificity and completeness of such formulations, it can be pointed out that fatigue, refractory phase decrement, etc., are conditions which may enhance gains during a rest period if they are present, but may be absent without thereby preventing the gains from occurring. Hovland's demonstration of reminiscence after only one presentation of a list is pertinent to this point.

*Differential forgetting.* The theory which now dominates the field of reminiscence has been developed by Hull and his associates and will be the only one reviewed in detail. As it pertains to the reminiscing of a single item, it is about as follows. In serial learning the presentation of successive items leads to the development of a chain of conditioned responses (42, 43) to the sequence of compound stimulus traces (38, p. 41). There are also remote excitatory tendencies arising from the conditioning of stimulus

traces to the various responses of the sequence (38, p. 45). The tendency to give a response is (presumably) re-enforced by the visual occurrence of it; at the same time an increment in inhibitory potential, affecting the excitatory tendencies operative then, is generated (38, pp. 51, 82). The algebraic sum of excitatory and inhibitory tendencies is the strength of the effective excitatory potential (38, pp. 24, 39, 62). The amount of inhibition generated in a massed practice is greatest at the center of a list (37; 38, p. 98), and therefore the strength of effective excitatory potential necessary to bring an item above the threshold of recall will be reached there later than at end positions.

The occurrence of reminiscence depends upon the more rapid loss of inhibitory than of excitatory tendencies bearing upon a particular response, during a period of no practice (38, pp. 65-69). An item just below threshold before the rest period may be reminisced because this differential in decay can, for a brief period, permit an effective excitatory tendency strong enough to bring the item above threshold (38, pp. 118-121, 256 ff.). The greater the amount of inhibition relative to the strength of excitatory potential before rest, the more chance there is that effective excitatory potential will increase enough to bring a response above threshold as decay of potential proceeds, provided of course that the total excitatory potential is great enough to permit this. Thus more reminiscent items are to be expected in central positions in the list than in end positions, and so on.

In the discussion of the relation between learning technique, subject matter and amount of reminiscence, it was indicated that there are three criteria as to the existence of more inhibition in one list than in another. Our first critical comment concerning the theory was incorporated in that discussion, since it seemed appropriate there. To repeat: the studies of Melton and Stone (55) and McClelland (46) show that these criteria can be satisfied, wholly or partially, without the appearance of improvement in a delayed recall. The direct dependence of reminiscence upon the disappearance of inhibition as inferred in this manner to be present has yet to be demonstrated. Because most of the theory depends upon this demonstration, it seems very important that more work be expended upon it.

The theory requires that remote excitatory tendencies arise during practice, and presumably they should be weaker than the direct excitatory tendencies. McGeoch (49), however, found no

indication of this in a retroactive inhibition experiment. This type of experiment does not indicate, in a manner which is crucial for reminiscence theory, just what happens to the remote tendencies, but it emphasizes again that the basic assumptions rather than the superstructure of the differential forgetting theory should be the most immediate topic of investigation. McClelland (46) has argued that error tendencies which *S* brings to an experiment, at any strength greater than zero, can not be treated by the theory.

The apparently greater tendency for reminiscence to occur in the center of a list rather than in end positions is considered to be proof of the fact that assumptions concerning the relatively greater amount of inhibition generated there are justified. Ward (68) has suggested that it actually is not possible to separate the effects of serial position as such and degree of mastery as such upon reminiscence. The occurrence of reminiscence in center positions might be due to the fact that this is the only part of the list not already mastered to a high degree. One can interpret the latter statement as Hull *et al.* (38) do, *i.e.*, the lower degree of mastery is due to the presence of inhibition, or it might be interpreted, as suggested by McClelland<sup>12</sup> by saying that *S* attends first of all to the end positions in the list and only later attempts to extend his memory span to center items. The interpretation is, then, that the concept of inhibition is not needed to explain serial position effects during learning, and the locus of reminiscence ought to be related to the possibilities of improvement at various positions in the list.

Although the actual occurrence of reminiscence in greater amounts in center positions is not to be doubted, for 12-item syllable lists, the reviewer has data (10) which do not show the tendencies exhibited by the shorter lists. They come from an unpublished study employing 16-item syllable lists. The Ward design was duplicated in all but two details—self-correction and pronunciation were required of *S*. All lists were learned to a criterion of one perfect trial before the immediate (6 sec.) recall or the delayed (2 min.) recall. The mean number of items recalled at the control interval for positions 7–12 inclusive was 4.94, and at the experimental interval, 5.61 ( $t=1.97$ ;  $P=.07$ ). The mean number of items recalled at the control interval for positions 3–5 and 14–16 inclusive was 5.39, at the experimental interval, 5.94 ( $t=2.55$ ;  $P=.02$ ). The mean gain was greater but less reliable statistically in the central positions (for the whole list,  $t=3.01$ ;  $P=.01$ ). The

<sup>12</sup> In unpublished seminar notes which he kindly loaned to the reviewer.



conclusion that gains are greater in central positions is further brought in question by inspection of Fig. 3 of Melton and Stone (55). Shown there are numbers of errors in various positions of a 16-item adjective list at the 6 sec. and the 2 and 5 min. recalls. The list was learned at a 1.45 sec. exposure rate, to a criterion of 12/16 before rest. Although, for the total lists, there was no reminiscence, it is clear in the graph that such reductions in errors as occurred in the experimental intervals were for items in positions 2-5 inclusive and 16, while the increases were largest in positions 8-11 inclusive. These results are just opposite those demanded by the differential forgetting theory. One can only conclude here, as in the discussion of the criteria of inhibition, that until the actual locus of the occurrence of reminiscence within a list is more adequately stated, and related to the possibilities for improvement, the theory is a superstructure built upon as yet insufficient experimental findings.

Perhaps the most difficult problem attacked in the monograph by Hull and his colleagues is the formulation of statements about the nature and origin of inhibition. The postulates merely state that an increment of inhibition occurs every time there is an increment in excitatory potential. "Inhibition of delay" and "inhibition of reinforcement" are both mentioned by the theorists (38, p. 51) as names for this type of deterrent to acquisition. As the writers themselves point out, such hypostatization is not satisfying. Although neither is explicitly incorporated in the system at present, two sources of inhibition are suggested.

The first (p. 54) is based on the assumption that when a tendency to say  $y$  as  $x$  begins to be established, the saying of  $y$  (conditioned) conflicts with the reading habit which produces the saying of  $x$ , the response to  $x$  thus being inhibited. McClelland (46, 47) has developed this possibility further in his studies of discrimination learning, and speaks of it as a performance decrement. Essentially, McClelland's view is that during acquisition, especially if this is "forced," the true level of learning is not exhibited in  $S$ 's responses; a rest period reduces the amount of conflict and therefore performance improves. Such a statement will fit most of the facts fitted by the Hull theory, except, *e.g.*, the occurrence of reminiscence after only one presentation of a list (32), and it lends itself to motor learning and discrimination learning more readily than does the differential forgetting hypothesis. On the other hand, until a plausible interpretation of why conflict should be reduced

during a rest period is found, the theory is no improvement over one assuming inhibition of delay.

The other alternative is advanced by Hovland (31) and Gibson (28). The source of inhibition is conflict, again, but this time it springs from generalization on either the stimulus or the response side. The idea is attractive because it might bolster one of the weakest points about the whole system: it now is essentially a disuse theory of forgetting and as such is very difficult to integrate with findings on retroactive inhibition. Gibson has already demonstrated the wide range of phenomena to which the generalization-differentiation hypothesis applies, and if it can be developed to handle the question of why increases in differentiation occur during a period of no practice, it would be a much needed inclusion in the main body of Hull's system. Furthermore, it would be consistent with the interpretation of inhibition on the basis of competing reaction systems, as defended by Wendt (69) and Guthrie (30).

Köhler (41), with support in part from the work of Müller (57), has presented what seems at first to be a different type of reminiscence theory. It is based on the assumption that cortical traces of items which have been learned by massed practice have "halos" of current which interfere with differentiation between traces and therefore with recall; after a period of no practice these halos tend to be reduced, differentiation between traces improves, and recalls improves. Questions may be raised concerning these physiological speculations, and the lack of specific deductions for experimental test, but the interesting point is that in spite of "language" differences, the essential concept here, as in the differential forgetting theory, is intra-serial interference or inhibition. When the trace theory<sup>13</sup> is carried further, specific predictions from it may differ with those from the Hullian theory; until that happens, the two theories can not be considered mutually exclusive.

It will be noted that the criticisms of the differential forgetting theory are aimed not so much at the deductions from the theory as at its experimental foundations. Improvement of the theory depends first of all upon certain "low level" studies which deal with the measures of inhibition, the nature of remote excitatory tendencies, etc., and the empirical relation of these to the locus and amount of reminiscence.

<sup>13</sup> Another type of trace theory is developed by Martin (53). It is Lewinian (74) in character, but it is not discussed here because of its specificity to Martin's type of experiment (plus the fact that his results are open to question, as indicated in the discussion on experimental controls).

## SUMMARY

Reminiscence has been defined as an improvement in recall, where all practice after the original learning has been ruled out, and it is proposed that for the present it should be studied apart from phenomena of the distribution of practice. It seems probable that reminiscence is a wide-spread occurrence, but there is little fully acceptable evidence of the validity of this statement. To date, reminiscence as a dependent variable seems to be related to the type of learning technique employed, the type of subject matter, degree of mastery before rest, type of practice (degree of distribution), length of rest interval, and probably to serial position, rate of exposure of items and the amount of material to be learned. The differential forgetting theory is clearly the most significant attempt to relate these findings. The criticisms directed at this theory have all dealt with its experimental bases and not with its general structure. The reviewer feels that the present status of our knowledge about reminiscence is such that we must emphasize the fact-finding phase of the fact-theory cycle. He also feels that the now-you-see-it-now-you-don't character of reminiscence research provides a challenging and at the same time promising area for future work.

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## THE PSYCHOLOGICAL BACKGROUND OF INDUSTRIAL BROADCASTING

BY WILLARD A. KERR

*RCA Victor Division  
Radio Corporation of America*

Psychology as a dynamic science applying itself to many fields of human activity, frequently finds that it alone among the sciences can adequately evaluate and explain certain social and economic values to be derived from new technological developments. An electronic application which has become important during World War II and which may be expected to remain a part of the industrial scene after the war is the plant broadcasting system. Some of the most important problems associated with use of these systems are properly psychological problems. Since it is now reported (1) that hundreds of factories have installed industrial communication systems at costs ranging from \$250 to \$50,000, it becomes increasingly important that examination be made of the relevant scientific literature which will facilitate more comprehensive and efficient use of these systems. These plant public address broadcasting systems—frequently installed with studios resembling those of sizable radio stations—are used for broadcast of both spoken material and music.

The limitations of the scientific investigations in this field will be apparent from examination of this paper or from reading of industrial music research summaries (22, 23).

In the use of music alone, many problems face the industrial broadcaster—What kind of music is best for the employee doing a given kind of work? How frequently and for how long should music be played? Is the value of a given type of music constant throughout the work day? Would specially composed music be more effective in promoting euphoria and output than existing music of traditional types? While few studies throw light upon these and similar questions, available evidence, experimental and anecdotal, favor the generalizations that (1) Management appears to be in general agreement that music favorably influences output and happiness of workers. (2) Music should be adjusted to emerge at an

optimum intensity from the prevailing noise level. (3) Music is of most value to employees engaged in manipulative-dexterity and repetitive type work as opposed to employees engaged in work requiring intense mental concentration. Relief of boredom is one of its greatest values. (4) As yet there is no evidence that music facilitates the output of persons engaged in higher mental operations. (5) It is the general belief of persons working in the field that the employees' appetite for music should be kept sharp by playing music for relatively short periods (e.g. 15-20-minute) during those stretches of the work spell when boredom and fatigue are thought to be most intense. (6) Attitude surveys of industrial workers report strong beliefs in the desirable psychological effects of music. A "feelings about music" survey of 229 electrical workers receiving music found significant majorities who reported that music improved their feelings toward their associates, helped their nerves, helped them forget their worries, and helped them when tired or when doing wearisome, monotonous work. Factor analysis of these beliefs indicated that more than one attitude was being measured.

These remarks with reference to music have been made in order to help orient the reader to the general field of industrial broadcasting. This survey is concerned with the non-musical use of such factory broadcasting systems.

Carver (3) found auditory presentation to be more effective for simpler material and visual presentation more effective for more difficult material. This is in line with DeWick's (4) experimental conclusion, based on a well controlled study of 70 college students, that "Auditory presentation of advertising material is distinctly superior to visual presentation when the problem involved is the recall of products and their trade names after a delay of from five days to five months."

Chief non-musical values derived from use of such factory broadcasting systems are believed to be those related to maintaining closer and more direct contact between management and personnel, paging of key workers, making announcements and emergency calls, and improving employee morale by providing regular news broadcasts, programs of recognition, and other special programs. There is considerable reason to believe that such broadcasting (17) has a favorable effect on production. This paper attempts to survey research which may have theoretical or practical value for the improvement of current usage of industrial broadcasting systems.

One of the first model plant broadcasting system studio layouts is that which was planned and constructed at DePauw University

by Professors Fay and Middleton and a group of RCA engineers. While combined with an experimental psychology laboratory, the DePauw studios, described elsewhere (10), embrace many fine features which could well be reproduced in industrial studios. Model industrial studios include those at the Botany Worsted Mills, Passiac, N. J., and at RCA Victor, Camden, N. J. The usual equipment provides for sending different programs to different floors, work areas, and buildings. A paging call likewise may be restricted to a specific building or floor. Several speaker types are available.

#### QUALITY AND CHARACTERISTICS OF SOUND

Present evidence indicates that broadcast material should be carefully presented in order to receive positive affective responses from the working listeners. In a study of noises, Laird (24) found that they reduced production in a laboratory "factory" employing two workers. Varying noise and high pitch noise were both more detrimental than their opposites. While these were meaningless noises such as those produced by an audiometer, it is possible that the conclusion regarding pitch may be related to Cantril and Allport's (3) finding that male voices are preferred for most purposes. Rosenblith (27) reports that hearing loss due to industrial noises is localized almost completely in the region above 1500 cycles with a maximum at 6000 cycles. Speaker outlets in the various work areas of a factory should be adjusted to prevailing noise levels. It is reported by Halpin (17) that in the weave shed at Botany Worsted Mills where the noise level is 102 db "Management says air raid instructions, paging, and music are heard reasonably well." Such intense noise levels are not common in the average factory.

#### CHARACTERISTICS OF ANNOUNCERS

Two studies (3, 9) have indicated that male announcers are usually preferred over women announcers. Fay and Middleton (9) conclude with reference to their study on college students that "Some women are preferred to some men announcers, but only when the men have relatively unappealing voices . . . men are even preferred for advertisements of women's products." In a study of 1075 radio listeners, Cantril and Allport (3) obtained the following responses to these questions. "Which type of *male* voice do you prefer to hear over the radio?" Tenor 31%, baritone 62%,

bass 7%. "Which type of *female* voice do you prefer?" Soprano 25%, alto 75%. The same writers report that "... the public despises 'affected' voices and on the average program dislikes feminine announcers." They found in one experiment however that women announcers are preferred for reading poetry and reflective, abstract material.

Fay and Middleton (5, 6, 7, 8, 11, 12, 13, 14, 15) have found in a series of experiments that student listeners discriminate better than would be expected by chance between announcers on the specific traits of persuasiveness, intelligence, sociability, pleasantness of voice, enthusiasm, sales ability, and occupation. These listeners were also able, too often to be explained by chance, to detect lying from the voice as transmitted over a public address system. In a similar study by Herzog (18), 2700 Vienna radio listeners judged height, weight, age, sex, and occupation of each of several radio speakers more accurately than could be accounted for by chance. Bonaventura (2) used six speakers selected for Kretschmerian morphological type and give their photographs to a large number of listeners. The auditors succeeded in matching photos with voice with considerable accuracy, pyknic being judged most accurately. Taylor (28) found a high degree of judge agreement in judging personality traits from voice.

Moses (25) has pointed out that in his clinical experience "the predominance of 'major' or 'minor' in the voice suggested predominantly optimistic or pessimistic attitudes." He also noted that in pleasure the inspiration tends to be short while the expiration tends to be long but that in displeasure the opposites are frequently true.

Significance of these and similar studies lies chiefly in the emphasis which they imply should be placed upon the careful selection and training of announcers. Most of these studies agree in finding that many voices are responded to as stereotypes.

When the announcer is to be "personalized," there is some evidence to support the adoption of career names when the announcer's original name is not pleasant. Walton (29) studied 285 men's and 298 women's names and found that the four men's names which are regarded as most pleasant are Robert, Richard, Jack, and Charles, while the four most pleasant for women are Jean, Jane, Virginia, and Dorothy.

Limited evidence indicates that the announcer should be a



male, but if the announcer must be female, she should possess an alto voice. If the announcer is to have control of the selection and arrangement of musical programs, he should have an appreciative knowledge of various types of music and he could profitably be trained in the measurement of the music preferences of employees. Editing ability for the arrangement of news bulletins is a definite asset.

#### CHARACTERISTICS OF PROGRAMS

Types of programs which have been suggested for various industrial situations include plant and company news, local, national and international news, sports news, special programs for national holidays, employee marriages, engagements, military enlistments, birthdays, local talent programs, regular recorded music programs, and educational programs. While the practicality of various non-musical programs will be limited by the conditions of a given industrial situation, the determination of exactly what is most practicable in various situations is a fertile field for experiment. Paging of key personnel and making of announcements in both office and production sections of a factory are now common over these communication systems.

In broadcast of verbal material, Carver (3) found that passages in which a general idea was followed by a specific reference or concrete illustration were in every case more interesting and better recalled than either an entirely general or an entirely specific passage. Carver also concludes that short sentences have greater memory value when the material lacks intrinsic interest or is of a highly factual nature; length of sentence does not matter for interesting material. The same experimenter reports that "The majority of broadcasts are most comprehensible and most interesting when the speed is not less than 115 nor more than 160 words per minute." Factual or difficult material can be broadcast more slowly without losing its interest, but readily comprehended material (news, etc.) loses its interest and is therefore less well understood if broadcast at a rate less than 120 words per minute. In general, repetition facilitates comprehension and aids memory, though it runs the risk of making a broadcast less interesting. Most suitable length for most educational, factual, or news broadcasts is about 15 minutes. Carver declares that "findings tend to indicate that the listener's loss of interest in long talks tends to

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counterbalance any intrinsic effectiveness gained by expansion in length."

Gaskill and Holcomb (16) conclude from their study of radio advertising that "More program content than advertising content is remembered . . . there is no relation between amount of time spent in advertising and its memory value . . . Greater memory value for advertising content seems to be directly affected by the ingenious 'sandwiching' in of advertising announcements. Complete isolation of advertising announcements, such as introductions and conclusions distinctly separated from the program seem to make for low memory value." It might appear from this that such utilization of the principle of affective assimilation could make ordinarily colorless or even unpleasant announcements or news seem somewhat more acceptable.

From a questionnaire study of 1,075 rural and urban Easterners equally balanced for sex, Cantril and Allport (3) found that in the rating of 42 types of programs, these ten stood highest in this order: old song favorites, dance orchestras, news events, symphonies, football, drama, humorists, sports, educational talks, psychology (latter two tied).

In the future, judging from present developments it will be possible to arrange an industrial broadcasting program with content types that will please practically all listeners. In a factor analysis of the preferences of 146 Princeton University students for 18 types of radio programs, Robinson (26) obtained three factor clusters (with item loadings greater than .40) as follows.

I. Drama factor: sports events, sports news, comedy and variety, dramatization of historic or scientific facts, dramatized news, dramatic plays, quiz programs.

II. Inspirational factor: forum talks and discussions, amateur hours, religious programs, news commentators, dramatized news, dramatizations of historic or scientific facts, folk and band music, serial stories, programs on personal problems, sweet dance music.

III. Unnamed factor: serial stories, programs on personal problems, serious music.

This same type of program type analysis should be made for industrial broadcasting programs. Although available evidence (19, 20, 21) seems to show that music programs are liked by an overwhelming majority of industrial workers, sports news, dramatic programs, and similar types probably have much less general appeal in the factory situation.

## SUMMARY AND CONCLUSIONS

While many of the conclusions to follow are tentative and deserve further investigation, especially with specific reference to the industrial situation, it is believed that some of these findings may be subjected to immediate practical application.

1. Public address broadcasting systems, with central control studios, are useful in the industrial situation.

2. For presentation of non-complex material, the spoken word appears to be more effective than the written word.

3. Both meaningless noises and speech of high pitch seem to be less pleasant and less desirable to the average listener than speech or noises of a lower pitch.

4. Most listeners usually prefer to hear a male voice; tentative evidence indicates that even women prefer male voices.

5. Listeners regard many voices as stereotypes, tending to associate certain personality characteristics with certain voices.

6. Certain names are regarded as more pleasant than others. While the affective weight of a pleasant name may not be great, the fact may justify adoption of pleasant names by "personalized" announcers.

7. Music probably stands highest in the list of favorite program types.

8. In broadcast of verbal material, passages in which a general idea is followed by a specific reference or concrete illustration are generally more interesting and better recalled than either an entirely general or an entirely specific passage.

9. Short sentences seem more effective for material lacking in intrinsic interest.

10. Optimum speed for most broadcast speech is probably between 115 and 160 words per minute.

11. Short announcements may be highly effective when "sandwiched" between pleasant stimuli such as music.

12. Programs, especially music, may be broadcast successfully through relatively high factory noise levels.

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## McGEOCH'S PSYCHOLOGY OF HUMAN LEARNING

### A SPECIAL REVIEW

BY DAEL WOLFLE

*University of Chicago*

McGEOCH, J. A. The psychology of human learning—an introduction. New York: Longmans Green & Co., 1942. Pp. xvii + 633.

John Alexander McGeoch (1897–1942) received his Ph.D. in psychology from the University of Chicago in 1926 while a member of the faculty at Washington University, St. Louis. Two years later he became Professor of Psychology at the University of Arkansas and then, successively, chairman of the departments at the University of Missouri, Wesleyan University, and the University of Iowa. He was cooperating editor of the *Psychological Bulletin* from 1931 to 1934 and editor from 1935 to the time of his death. During the last dozen years of his life he was elected to a number of offices in regional and national professional associations.

McGeoch started contributing to the literature of psychology at the age of 22 with the publication of his Master's thesis: "The present status of psychology." From then until the writing in 1926 of his Doctoral dissertation ("A study in the psychology of testimony") his interests ranged over intelligence tests, imagination, suggestibility, perception of filled and empty time, and the fidelity of report of normal and subnormal children. From 1926 until his death practically all of his research work and publications were centered in the study of human learning. During this time he published some 60 articles, wrote chapters on learning for several texts and handbooks, and, as a final review of his chosen field, *The psychology of human learning*.

The book McGeoch wrote was not, however, the one he wanted to write and which he, probably better than any other psychologist was prepared to write. For years he had been reading, reviewing, criticizing, and abstracting the learning literature, intending some day to write a complete and definitive handbook of human learning.

Carr recounts, in the preface, some of the difficulties of that venture. McGeoch's chapters, Carr's extensive criticisms, and McGeoch's revisions accumulated too slowly, and were interfered with too much by McGeoch's ill health and by other demands on his time. Finally McGeoch decided to write a briefer introduction to the subject, a single volume which might be used as a textbook and which would give him the practice in organizing and condensing his voluminous notes which he wanted as a preliminary to the writing of the larger handbook.

The book which we have was therefore regarded by McGeoch as a textbook and as a predecessor to his major work. It consists of 14 chapters. The first two are general in nature, covering "Concepts and Methods," and "Curves of Learning." These two chapters define and illustrate a number of the concepts of learning and its measurement. The pervasiveness of learning, the difficulty of arriving at a rigorous definition of learning, mazes, nonsense syllables, methods of measuring retention, conditioning, association, motivation, the Vincent curve, plateaus, and different curve forms are some of the topics covered.

Then follow nine chapters devoted to discussions of specific learning problems. These are: "Intraserial phenomena," "Distribution of practice," "Learning as a function of the material learned and of certain modes of practice and presentation," "Learning as a function of chronological age, sex, and test intelligence," "Learning as a function of motive-incentive conditions," "Retention," "Conditions of retention," "Transfer of training," and "Fundamental conditions of forgetting." Each of these nine chapters contains a systematic factual summary of some aspect of learning. Most of the emphasis in each chapter is given to experimental findings, but in addition relevant theories and any methods of experimentation specific to that subject are described, and a little, but not very much, of the relations to other topics may be given.

Since each chapter is fairly complete by itself, they may be read in any order. A student interested in a particular topic can read the appropriate chapter without loss of understanding through having failed to read the earlier chapters.

The three final chapters, again more general in nature, are "Fundamental processes and conditions of learning," "Fixation and elimination: frequency and related variables," and "Fixation and elimination: the empirical law of effect." These chapters deal with a typical process of learning, analyzing it into a problem

situation, the discovery of an adequate solution, the fixation of the responses involved in this solution, and the elimination or inhibition of less adequate responses" (513). Trial and error, insight, the role of transfer, and the importance of contiguity are the most important topics in the first of these chapters. The two final ones discuss the roles of frequency and effect in determining which responses will become fixated and which will be eliminated or inhibited.

The book has two features which distinguish it from a book on learning that many another psychologist might have written. First is the restriction to the field of *human* learning. The differences in methodology, in empirical findings, and in theoretical explanations between human and animal learning are too small to permit the complete exclusion of animal data; every once in a while McGeoch slips in the results of an animal study. But throughout, he has tried to keep the discussion at the level of human learning. In so doing he has minimized or excluded entirely the treatment of conditioning, of maturation, of physiological studies of learning—fields in which most experimenters have used animal subjects.

The second distinctive feature, like the first, reflects McGeoch's own selective interest in learning. His interest, and the interests of those with whom he worked most closely and who had the greatest influence on his thinking—chiefly Professor Carr—were primarily empirical. In this respect McGeoch is a conscious follower of Ebbinghaus to whom he gives credit not only for starting the "systematic experimental study of human learning" but also for setting the pattern for much of the later work in the field.

The empirical interest which marked McGeoch's thinking determined to a very large extent the authors from whose work he drew in writing his book. Nearly half of the men most frequently quoted by McGeoch were, like himself, Carr's psychological children from the University of Chicago—Bunch, Hunter, Pechstein, Peters, Joseph Peterson, E. S. Robinson, Stroud, Warden, Waters, and others. A smaller group of frequently-quoted authors, mostly students and colleagues of McGeoch himself or of E. S. Robinson, are, to varying extents, the second-generation product of Carr's extensive influence. There are, of course, frequent references to the work of Ebbinghaus, Guthrie, Hilgard, Hull, Köhler, Müller, Thorndike, Woodworth, and one or two others who are necessarily quoted in any book on learning, but references to many compara-

tive psychologists who have contributed important learning studies are only infrequently made.

The result of this selection on McGeoch's part is a book containing much about retroactive inhibition, maze and nonsense-syllable studies, forgetting, memory traces and remote associations, the law of effect, and the like, but a book influenced much less than most recent volumes on learning by conditioners, behaviorists, physiologists, gestaltists, or mathematico-deductive theorists. Adherents of these varied interests and points of view will frequently find their work quoted by McGeoch, but in general just the results are given. The theoretical background responsible for the experiments is usually missing or only sketchily appended to a summary of results.

The book is much like McGeoch's own experimental studies in content, method, and point of view. While it resembles a research monograph in this respect, it is not really that; to think of it as merely a review of the author's own research is altogether too narrow a conception of its scope. Even though deliberately condensed it will serve the professional psychologist better than anything else available as a substitute for the larger and more complete handbook that McGeoch wanted, but did not have time, to write.

One can not read *The psychology of human learning* without a feeling of sadness, for in its author's death psychology has lost an able and vigorous worker, and psychologists a genial friend. Yet we may be glad that he was able to complete this summary of the field to which he had devoted the whole of an all too short professional career. McGeoch has given us, as his last contribution to psychology, a simple, clear picture, presented without adornment and without apology, of what he considered the most vital, most central part of the whole field of psychology.

## NOTICE

### THE FIFTY-FIRST ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, SEPTEMBER 2, 1943

*To Associates and Members of the American Psychological Association:*

The *Preliminary Announcement and Call for Papers* is usually issued by the Executive Committee and the Program Committee in April. The Program of the Annual Meeting has usually been made up in May and June and published in the July issue of the *Psychological Bulletin*. These steps were carried through in 1942 although the meeting planned for Boston and Cambridge was subsequently cancelled and a skeleton meeting held in New York City to transact essential business.

At the Annual Meeting in New York City the Association voted: "That the next Annual Meeting be scheduled tentatively for Thursday, September 2, 1943 at Chicago, it being understood that it will be a skeleton meeting unless restrictions on travel are raised, that Council may change the plan by a declaration of emergency or by the exercise of interim powers, and that Council be authorized to name a local member of the Executive Committee."

The Program Committee, consisting of Harold Burtt, Chairman, Dael Wolfle, and the Secretary, has kept in touch with the Announcements of the Office of Defense Transportation and has engaged in a large amount of correspondence concerning the possibilities of an Annual Meeting. As a result of these activities recommendations were transmitted to Council for review.

It is the combined judgment of the Program Committee and Council that no Call for Papers should be issued for the 1943 meeting. It has been agreed that the only official program to be announced at this time will be the Annual Business Meeting on the afternoon of Thursday, September 2, at some point in the Chicago area to be determined later. The intent is that the meeting should be attended by Council and by Members in the vicinity whose presence would not place an added burden on the transportation facilities of the country.

Further announcements will be made by mail or through the *Psychological Bulletin*.

WILLARD C. OLSON, *Secretary*



## PSYCHOLOGY AND THE WAR

Edited by

STEUART HENDERSON BRITT

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## PERSONNEL RESEARCH IN THE ARMY

### IV. THE SELECTION OF RADIOTELEGRAPH OPERATORS

BY STAFF, PERSONNEL RESEARCH SECTION, CLASSIFICATION  
AND REPLACEMENT BRANCH, THE ADJUTANT  
GENERAL'S OFFICE

The highly mobile character of the present war, with its necessity for maintaining communications, has created an unprecedented demand for radiotelegraph operators. They are needed in almost all of the Arms and Services, such as the Signal Corps, Infantry, Air Forces, Armored Forces, Cavalry, and Field Artillery. So great is the number required that nearly five per cent of the men taken into the Army are trained as radio operators. Naturally, only a small part of this total is supplied by men coming from civilian life with the needed skills. In times of peace, relatively few individuals make a livelihood in this field and the number of amateurs is not large. The Army, then, is faced with the problem of training the specialists that it needs. Therefore, because of the large numbers of men involved and the shortage which exists, the selection of radiotelegraph operators has become one of the most important problems with which the Personnel Research Section is concerned.

Stated in broad form, the problem is one of selecting men who can be trained to become efficient radiotelegraph operators in the shortest possible time. Viewed in the context of the Army situation, however, the apparent simplicity of the problem vanishes. A review of the most outstanding complexities is essential to an evaluation of the results reported later. These may be discussed under three main heads: first, securing adequate criteria; second, generalizing results from one branch of the Army to another or even from one training installation to another; and third, securing adequate control of the conditions under which the data for the evaluation of selection instruments are determined.

What are the characteristics of an "efficient" radiotelegraph operator? The answer to this question may vary considerably, even within a particular Arm or Service. Ability to send and re-

ceive code is certainly basic, but the qualifying speeds may go as low as 9 words per minute (w.p.m.) or as high as 25 w.p.m. depending on the kind of net in which the operator will work. Besides code, the student operator must learn a large amount of non-code material. This may include printing, typing, operation of typical radio sets, message procedure, use of other communication devices such as visual signaling, blinker, panels, flags, and pyrotechnics, use of camouflage and cover, identification of and defense against enemy aircraft, packing and moving equipment, reading maps and aerial photographs, sketching terrain features, and use of elementary cryptography. The non-code material taught in a course varies widely, depending on the purpose for which a particular group of students is being trained. It is thus impossible to define an "efficient" radio operator in general terms that would be applicable to all or even most of them.

*Reported Reasons for Failure in Radio Courses and the Problem of Selection.* In view of the speed with which radio operators must be trained, and the definite standards of excellence which they must attain, it is to be expected that an appreciable number of failures will result. This expectation has been fully realized, and emphasizes the need for improved selection techniques. Evidence on the relative influence of code failures and other sources of failure as reported by training organizations is useful in determining the amount of emphasis to be given to code in selection problems.

In one breakdown of reported cause of failure for 275 Air Corps Communications students who had been eliminated, it was found that about 63% were eliminated for code failure as compared with 16% for failure in other subjects. Another study was based on 406 failures and "washbacks" who failed to complete the training in the allotted time. Of this group, low code speed was given as the only reason in 31% of the cases, while failing grade in radio mechanics was the only reason given in 15% of the cases. Since, in this set of data, more than one reason for failure was given in many instances, it is worth noting that low code speed was involved in about half of the failures, while failing grades in radio mechanics appeared as a reason in about two-fifths of the cases. The remaining causes included mainly continued absence, hospitalization, and other situations not amenable to prediction.

Reports from other training centers indicate that the relative importance of mastery of code in determining success or failure is even greater than found in the studies reported. As a result, the efforts in selection have been primarily directed toward measuring aptitude for learning code. Mastery of code involves both the

ability to send and to receive. Attention has been focussed upon receiving, however, because ability to receive code is generally considered a prerequisite for learning to send, and because the two abilities are so closely connected that the number of students who pass receiving but fail sending is not large enough to warrant a specific attack on this problem.

*Criteria of Success in Code Learning.* Since the mastery of the International Morse Code is of such central importance in radio operator training, special interest attaches to the choice of a criterion of success in this skill. The urgency of the need for operators has led to an emphasis upon speed of learning code as the underlying variable to be estimated from the data available. The measures have taken two general forms: first, the amount of progress attained within a specified period of time; and second, the amount of time required to reach a given level of attainment.

A brief examination of the characteristic procedure in code training will aid in the evaluation of the criteria used. After a student has learned to identify the alphabet and numerals, he is advanced from speed to speed in roughly uniform steps. For example, a student who has qualified at the 5 w.p.m. speed, would be moved up to the 7 w.p.m. level, and would normally continue to advance until the end of the training period. Since students are usually passed or failed in code on the basis of the highest speed passed during the course, the final code speed attained is usually available. This criterion has been used in a number of studies. It produces however, a rather crude grouping of the observations, especially where the steps are large. It may suffer from a more serious difficulty, namely, that the number of hours of actual code training may vary appreciably within a group, because of illness, late assignments, and other causes. It is also more difficult to compare results from different schools which allow varying amounts of practice within the training program. A different form of this criterion provides code speed attained during specified intervals of time, as at the end of 4, 8, 12, and 16 weeks. While this provides additional data of some value, it does not escape the difficulties mentioned above.

It is apparent that a more refined measure would be provided by the number of hours required to attain the various levels of code performance, and this criterion has been used whenever such data have been obtainable. The results of a validity analysis are more complex, in that more correlations are determined, but these additional correlations are useful both in interpreting the study and in comparing results with other studies. A minor inconvenience of this criterion is that the validity coefficients are negative.

The second problem, that of generalizing results from one



branch of the Army to another, is apparent from the above discussion of variations in requirements of operators trained for different purposes. The third problem, that of securing adequate control of training conditions, is closely related to the second. If men are being trained for different purposes, it is natural that their training will vary. With regard to the teaching of the code material itself, the instruction will not vary as much as the above statement might lead one to expect, but sufficient variation does exist so that care must be exercised in the comparison of data from different installations.

The manner in which men are selected for training is the most important point to keep in mind in the interpretation of data. Almost without exception, the men given training as radiotelegraph operators are a selected group. They are chosen, first of all, on the basis of their score on the Army General Classification Test,\* only those men in grades I, II, and III being chosen for training. For some types of radio operator training, still higher standards on AGCT are employed. Thus, individuals at the lower end of the scale on general mental ability are excluded. The correlation between scores on the Army General Classification Test and code speed is not high, but it is positive, and selecting men for training on the basis of AGCT scores tends to reduce the validity coefficients which are secured. A second selective factor that generally operates is selection of men on the basis of their score on the Radiotelegraph Operator Aptitude Test (ROA-1, X-1), to be described later, which is given to all men entering the Army who make a score of 80 on the Army General Classification Test. Only those men who have scored average or better on this test are generally chosen. In some instances, if many qualified men are available, only those scoring highest in the selective criteria will be trained. The practice of selecting men who are best qualified for training on the basis of measures of mental ability and code aptitude is an excellent one from the viewpoint of training, but its effect on instruments of selection is to make them appear less effective than they really are. This point must be kept in mind in appraising the results that have been secured in the studies reported in this article.

\* The Army General Classification Test, given to all men entering the Army, serves as the general mental ability test for classifying men. On the basis of the score made, an individual is given an Army grade from I to V, I being high. Scores on all tests are translated into Army grades. The top three grades include about 70% of the men.

*Tests Used in Predicting Speed of Learning Code.* Since the Signal Corps Code Aptitude Test (now called the Radiotelegraph Operator Aptitude Test, or ROA) was already in use for selecting radio operators when the expansion of the Army began, initial efforts were devoted to an analysis of this test. Many requests were received from training organizations for information regarding this test and a number of units collected data on it.

ROA-1, X-1\* is a specific aptitude test, which grades men on their ability to make auditory discriminations considered to be similar to those made by an air operator in receiving code. The test is somewhat the same in form as the Seashore Rhythm Test, except that the patterns of sounds which are to be judged "same" or "different" are patterns of typical code sounds—"dits and dahs." It is usually administered by means of phonograph records, but may be administered by use of tape recordings similar to those used in training operators, or even hand-sent by a skillful operator. The sounds are to be sent at a speed equivalent to a code speed at 20 w.p.m., and approximately 9 minutes are required for one presentation of the test. It will be noted that standard conditions are achieved only when the phonograph records are used.

Variation in item difficulty is produced by using sound patterns whose lengths range from 2 to 13 sounds and whose members differ, with the difference occurring at varying points within the pattern. The test contains 78 pairs of code patterns. Its reliability, estimated by Kuder-Richardson formula #21, is approximately .75. Studies reported in this article, except where otherwise noted, are based on a single administration of the test. However, in view of the shortness of the test and its low reliability, the test is now repeated with a two-minute interval between repetitions. At the present time, this double administration of the test is standard procedure in reception centers, and every man inducted into the Army who makes a score of 80 on the Army General Classification Test takes the test in this form.

The validity of the ROA has been investigated in a number of studies. However, most of the earlier studies included both experienced and inexperienced students. Evidence now available, some of which is reported later in this article, indicates that where an appreciable number of experienced operators are included in a study, the obtained validity of the test is markedly higher than that obtained when only inexperienced men are included. From a practical viewpoint, the main function of a selection test for radio

\* ROA-1, X-1 indicates the first form of the Radiotelegraph Operator Aptitude Test. X-1 indicates the first experimental form of form number 1. Various forms of a test are numbered 1, 2, 3, 4, etc., and subsequent revisions of a form are indicated as X-2, X-3, X-4, etc. When no more revisions of a test are contemplated, that is, when a test has passed out of the experimental stage, the X symbol is dropped.

code operators is to pick promising men (skilled men would be selected in any case); and from a statistical viewpoint, a population containing a mixture of experienced and inexperienced men is not homogeneous. Consequently, analyses of tests for the selection of radio operators now treat these two groups of students separately.

In an effort to obtain a test which would be more effective in selecting inexperienced men who would learn the code rapidly, various forms of a Code Learning Test have been devised and tried out. Essentially, the Code Learning Test involves teaching the student a portion of the code, and then testing his degree of mastery. Nine forms of the test have been formulated. In these forms, the method and amount of practice is varied, and on some forms, characters which have not previously been practiced are introduced into the test. The test is given from recordings played over a public address system or through earphones. Description of two of the forms follows:

CLT-2, X-3 begins with a learning period of 30 minutes during which the code characters for 6 punctuation marks are given. During part of the learning period a code character is announced and then presented. During the remainder, the code characters are presented in random groups followed by a voice which identifies each character. There are 180 items in the practice period. The test proper consists of 100 items. In it, each of the 6 characters learned during the practice period is presented 12 times together with 28 unlearned characters, all in random order. The examinee responds by recording the symbol for a character if it is one he has learned, or by a zero (0) if it is one not learned during the practice period. The reliability of this form of the CLT estimated by the split-half technique and Kuder-Richardson method is in the high .90's.

In CLT-3, X-4, 5 characters are learned, each associated with the number 1, 2, 3, 4, or 5, and responses in the test are recorded on a 5-place answer sheet for machine scoring. Practice test items are interspersed in the learning period, but the examinee has no opportunity to check the accuracy of his responses. One hundred items are presented during the learning period. The test itself consists of 105 items made up entirely of the 5 characters presented during the learning period. In all forms of CLT, the score is the number right.

Two other tests concerned with code and code characters have been used in studies of radio operator selection. The first of these is the Substitution Test, a paper and pencil test involving the learning of paired associates. The key contains 10 symbols, each with a corresponding letter. The test consists of filling in beneath each symbol in the test the letter with which it is paired. During a practice period of 3 minutes the subject can practice on 100 items.

The test itself, which has a time limit of 5 minutes, contains 209 items. Scoring is based on the number of correct responses. The reliability is in the high .90's.

The Code Rhythm Test is based on the Thurstone Rhythm Test and measures the ability of an individual to differentiate between the dots and dashes used in code and to remember code patterns of varying length. There are 5 short sample items and 45 test items with patterns consisting of from 5 to 13 dots and dashes. By use of a simple system of symbols, the subject reproduces each sound pattern presented. The score is the number of elements correctly recorded.

*Studies on Radiotelegraph Operator Selection.* In reporting the work that has been done on the selection of radiotelegraph operators, the discussion will concern 5 studies. These include most of the aspects of the problem already investigated. Several, and in some instances many, studies have been made on each of the specific problems discussed. Results of studies not reported are in substantial agreement with those presented. Material from other studies will be brought in when it is necessary to supply additional information.

The first study is the relationship of three tests used in radio operator selection (the Code Learning Test-3, X-3, the Substitution Test, and the Code Rhythm Test) and the relationship of each to previous radio operator experience and to education. Four hundred and seventy Air Corps Communications students were used in the study, but not all of the men took all of the tests so that the numbers of cases on the intercorrelations vary from 149 to 312. Experience in code was secured from a questionnaire on which levels of experience were assigned 1-point values from 0 to 6. Each subject checked the statement which best described his own level of experience. The 0 value statement was "Never had any experience with code," the 6th level "Have been a licensed operator." Those men with experience in code were asked to indicate their maximum speed of receiving.

The reliability of all three tests was found to be high, being .95 or above as estimated by the Kuder-Richardson method. The two estimates of experience (self-rating on the 7-point scale and estimated maximum speed) correlated .63. As might be expected, the Code Learning Test-3, X-3 and Code Rhythm Test showed a rather high correlation (.54) with each other, and both tests showed rather low correlations with the Substitution Test (.24 and

.09 respectively). Estimated experience correlated about .40 with the two auditory tests but negligibly with the Substitution Test. The correlations involving experience were about the same when the 7-point scale was used as when estimated code speed was used. The correlations involving years of schooling turned out to be uniformly low, varying from  $-.12$  to  $.12$ . The selected character of this group is indicated by the fact that for highest school grade completed, the mean was 12.54 and the standard deviation 1.68. (In another study of 1203 students, where the mean number of years of education was 10.65 and the standard deviation 2.40, the correlation with the radio operator course grade was only .23).

A second study, done in a different training center, permits the comparison of CLT-2, X-3 with the Substitution Test. The tests were validated against time to reach various levels of proficiency in receiving code (4, 8, 12 and 16 w.p.m.). There were 217 cases at the beginning—all students in the radio operator course. Of the total, 174 had no previous experience in code, while 43 had had some experience in receiving and sending. Estimated receiving speed for the latter group ranged from 4 to 15 w.p.m. For the computations, all experienced operators were grouped. Both groups of tests had estimated reliabilities of .97 for the total group. The correlation between this code learning test and the substitution test turned out to be .39 for the inexperienced group, .54 for the experienced, and .42 for the two sub-groups combined.

Table 1 shows the correlation coefficients between scores on the 2 tests and criteria of number of hours to receive code at 4 different speeds.

TABLE I  
CORRELATIONS BETWEEN SCORES ON CLT-2, X-3, AND SUBSTITUTION TEST WITH  
CRITERIA OF NUMBER OF HOURS TO ATTAIN 4, 8, 12, AND 16 W.P.M., RECEIVING,  
FOR STUDENTS IN RADIO TELEGRAPH COURSE.

	4 w.p.m.			8 w.p.m.			12 w.p.m.			16 w.p.m.		
	*I	*E	*C	I	E	C	I	E	C	I	E	C
CLT-2, X-3	-.46	-.63	-.68	-.44	-.60	-.65	-.38	-.59	-.60	-.42	-.56	-.57
Sub. Test	-.24	-.30	-.27	-.24	-.35	-.29	-.20	-.33	-.26	-.20	-.37	-.27
N	174	43	217	170	43	213	166	44	210	160	43	203

\* I = Inexperienced; E = Experienced; C = Combined.

This comparison indicates that the Code Learning Test-2, X-3 is distinctly superior in validity to the Substitution Test. It is also apparent that the validity of the CLT in the combined group is satisfactorily high, ranging from  $-.57$  to  $-.68$ . In interpreting these coefficients, it should be noted that students who were dropped for code failure before completing any particular code



speed were necessarily omitted in making the calculations. When inexperienced students only are considered, the validities are noticeably poorer, although high enough to justify further consideration of this type of test. The higher correlation in the combined group has appeared consistently in other studies of code learning and is to be expected. The mean score made on CLT-2, X-3 by the experienced men is significantly higher than that made by inexperienced men ( $C. R. = 3.8$ ), supporting the views that the combined group is not statistically homogeneous.

In a third study, scores on Code Learning Test-1, X-1,\* preference for receiving training as a radio operator, and experience in playing a musical instrument were evaluated in relation to the number of hours needed to attain a specified code speed (receiving). Musical experience was studied in relation to code learning as several instructors had noted that men who could perform on musical instruments learned code more readily than those who could not. Instructors also had stated that lack of interest appeared to be one of the chief reasons for failure in radio operator courses. Two hundred and three cases were included at the start of the study, none of whom had had any experience with code, and no cases were included that were dropped for any reason other than failure to develop code speed. All had made scores of 50 or above on Code Learning Test-1, X-1 and 90 or above on the Army General Classification Test, thus placing them in Grades I, II, or III. They were thus a highly selected group as is indicated by the fact that of a group of 2096 men scoring Grade III or above on the Army General Classification Test, about 35% made a score of 50 or above on Code Learning Test-1, X-1.

Coefficients were computed between scores on the Code Learning Test-1, X-1, and hours to attain code speeds of 2, 5, and 8 words per minute. These were  $-.46$ ,  $-.48$  and  $-.41$  respectively. Only 90% of the group beginning the training reached the code speed of 8 words per minute, the rest having been dropped for failure to develop code speed. The validity coefficients are almost the same as those reported for CLT-2, X-3 for inexperienced men in the preceding study, indicating that this kind of test is also a

\* CLT-1, X-1 is identical to CLT-2, X-3 (described above) in form and administration, the difference between them being the code characters to be learned. Four out of six of the characters in the two tests are different although the number of elements in each is the same and the two sets are of approximately equal difficulty.

fairly promising measure of ability to develop code speed. The correlation between score on the Code Learning Test-1, X-1 and the General Classification Test was .20. It is true that the group was selected on the basis of the Classification Test score, but the low correlation coefficient does indicate that within a group selected in this way the two tests are largely independent.

Musical instrument experience was defined as ability to perform on musical instruments before groups. The biserial correlation between musical instrument experience and time to receive code at a speed of 8 w.p.m. was  $-.24$ , an unreliable value, being only about twice its standard error. The number of hours necessary to reach this standard of 8 w.p.m. is about 5 hours for those without musical experience, but the amount of overlapping was so great that little significance can be attached to this fact. A biserial correlation of musical instrument experience with a score on Code Learning Test-1, X-1 of .40 indicates that if selection is made of men on the basis of the Code Learning Test, individuals with musical instrument experience are more likely to be chosen for training, even if no direct consideration is given to that characteristic.

Preference for receiving training as radio operators was indicated by the students at the completion of the Code Learning Test. They had at least some idea as to the kind of work involved in mastering code. However, the biserial correlation of preference (and non-preference) was small and unreliable both for code speed and for code learning test performance. This result suggests that *initial* preference is not a dominant factor either in test performance or in later mastery of the code.

Related data on the relationship between musical ability and code learning were secured when the Seashore Tests of Musical Talent were administered to two groups and the scores correlated with code speeds (receiving) developed after specified numbers of weeks of practice. With few exceptions, the resulting coefficients were positive, but all were low, those with the tests on Pitch and Rhythm tending to be higher. These, however, were lower than the correlation coefficients generally secured with the Radiotelegraph Operator Aptitude Test (ROA-1, X-1) or any of the Code Learning Tests. The Seashore Tests show about the same correlation to code speed that exists between code speed and experience on a musical instrument. Neither is sufficiently high to justify its use in the selection of radio operators.

A fourth study, in which records were kept on time to attain

code speeds of 4, 8, 12, and 16 w.p.m. for 8 different classes in the same installation, makes possible a comparison of Radiotelegraph Operator Aptitude Test-1, X-1 and Code Learning Test-2, X-3 as predictive devices. The classes varied in size from 66 to 272 men, including in all 1365 cases. All men took ROA-1, X-1 twice and the sum of the scores on the two administrations is used as the score. Three of the classes were given CLT-2, X-3 twice and the average of the scores of the two administrations used. The other classes were administered the test only once. For two classes correlations were computed between time to attain code speed at various levels and scores on single and double administrations of the CLT-2, X-3.

All of the students in the study were without previous experience in code. In all classes but one, the number of men on whom data were available decreased as the class progressed to higher code speeds. The percentage of failures, however, was not great, varying from 5 to 10 per cent of the men starting in each class. A summary of the data is shown in Table II.

TABLE II

MEAN TEST SCORES AND CORRELATION COEFFICIENTS OF CLT-2, X-3 AND ROA-1, X-1 WITH HOURS TO ATTAIN CODE SPEEDS (RECEIVING) OF 4, 8, 12, AND 16 W.P.M. FOR 8 CLASSES OF RADIOTELEGRAPH OPERATORS. (INEXPERIENCED MEN ONLY)

Class No.	N†	4 w.p.m.		8 w.p.m.		12 w.p.m.		16 w.p.m.		Mean Scores†	
		CLT	ROA	CLT	ROA	CLT	ROA	CLT	ROA	CLT	ROA
1	240	-.40	-.38	-.38	-.32	-.36	-.30	-.40	-.25	43.16	101.07
2	162	-.45	-.30	-.33	-.33*	-.34	-.32	-.33	-.28	51.87	109.13
3	187	-.53	-.24	-.44	-.29	-.35	-.24	-.33	-.26	52.13	110.17
4	272	-.34	-.11	-.36	-.20	-.37	-.16	-.28	-.14	41.31	106.91
5	190	-.40	-.31	-.34	-.34*	-.32	-.41*	-.29	-.33	52.24	111.37
6	102	-.46	-.11	-.49	-.17	-.55	-.19	-.56	-.13	37.67	110.50
7	65	-.55	-.33	-.54	-.58*	-.49	-.48	-.54	-.53	59.77	114.29
8	127	-.49	-.34	-.44	-.21	-.32	-.13	-.22	-.14	39.77	107.35
Median		-.46	-.30	-.41	-.30	-.36	-.27	-.33	-.25		

\* Indicates ROA-1, X-1 coefficients that are higher than corresponding CLT-2, X-3 coefficients.

† Number of cases and mean scores of students passing 4 w.p.m.

In this study, as in others that have been conducted, the Code Learning Test correlates more highly with code speed at various levels than does the Radiotelegraph Operator Test. In only a few cases is the coefficient of code speed with ROA-1, X-1 equal to or higher than the corresponding coefficient for CLT-2, X-3. The range of correlation coefficients computed with scores on each test at each criterion level varies widely. Within each class, however, the coefficients are relatively consistent from one code speed to another. This would indicate that the differences which resulted

were due to differences among the classes themselves, a fact which is borne out by the wide variation which exists in the initial mean scores made on both the tests. It is possible that criteria were used in selecting the men that were not reported with the data, or that were impossible to determine. Thus, when one class was formed, men with high scores on the Army General Classification Test might have been available; at another time, men with higher scores on the Radiotelegraph Operator Aptitude Test. Other things being equal, the best men available for any training are always taken, and due to varying demands, the calibre of the available men on different measures varies from one week or month to the next.

In the two classes where correlation coefficients were computed between the validity criteria and the sum of scores on two administrations of CLT-2, X-3, the validity of the test was increased by an amount ranging from .02 to .08, except for one coefficient which dropped .01. On the whole the gain in validity was judged insufficient to justify the repetition of the test.

Intercorrelations of scores on CLT-2, X-3 and ROA-1, X-1 computed for the various classes varied from .25 to .56. Again, the range of coefficients is wide. In other studies, the correlations of scores between the two tests have hovered around .40, indicating some degree of overlapping between the tests. Multiple correlation coefficients for the various groups are not markedly higher than the CLT validities.

The code learning test, which attempts to sample the job involved in the training itself, appears to be definitely the most promising approach at present to the problem of selecting inexperienced men who will master the code most rapidly.

The last major study reported in this article was concerned with a determination of tests that would be useful in predicting grades in electrical courses, given as part of the radio operator training, as well as code speed. Scores on 28 tests, grades in 5 electrical courses, 1 mathematics course, and code speed at 8 weeks (receiving) were secured on a group of 86 students in a communications course. The tests on which scores were available included the Army General Classification Test, Radiotelegraph Operator Aptitude-1, X-1, the Seashore Tests of Musical Talent (already commented on), four mechanical information tests, viz. Shop, Electricity, Physics, and Mathematics, United States Employment Service Test, C-40-A, from which part scores on radio and electricity were secured, Physics test (Columbia), 5 tests from the Thurstone tests of pri-

mary mental abilities, 3 Brother Phillip tests, and several tests on surface development and mechanical movements. Electrical course grades were on the following: Direct Current, Alternating Current, Transmitters I, Transmitters II, and Receivers. Course grades were used as validity criteria for non-code materials of radio operator courses.

Correlations were computed between all the tests and code speed at eight weeks. The highest value was with ROA-1, X-1, the coefficient being .59. The number of experienced men in this group is not known. The Letters Test\* (an Air Corps Test) correlated .40 with code speed. The Seashore Pitch and Rhythm tests both were .39. Correlation with Number Span Test (Brother Phillip) was .33, with Sustained Attention Test (Wittenborn)\* and Seashore Tonal Memory, .31. All the rest of the coefficients were below .30, and with but one exception, all positive. AGCT correlated .16 with code speed in this group, but the sample on AGCT was highly selected. Correlation with the four electrical tests\* ranged from .05 to .21, giving further evidence of the lack of relationship between the code and non-code aspects of training indicated in the reasons for failure reported earlier in the article.

Correlations also were computed between all the tests and the five course grades. Eleven of the tests showed promising correlations with electrical course grades, so additional coefficients were computed between them and the average of the five course grades. The eleven tests showing best relationship with grades in electrical courses ( $r = .41$  to  $.68$ ) were the Army General Classification Test; Mechanical Movements (Air Corps); General Electrical Information (Signal Corps); United States Employment Service, C-40-A Radio (part score); C-40-A Electricity (part score); C-40-A (remaining parts); the mechanical information tests, Shop, Physics, Electricity, and Mathematics; and Physics (Columbia). Of these eleven, four—the Army General Classification Test, C-40-A (Radio), mechanical information, Electricity and Mathematics sections—gave a multiple correlation coefficient of .75 with the average grade of 5 electrical courses. The four tests can all be

\* The Letters Test is a verbal digit-symbol test, in which the letters of the alphabet are given verbally to the subjects who record the code symbols for these letters. In the Sustained Attention Test the subject is required to check in a specified manner combinations of vowels and consonants presented irregularly. The four electrical tests are: Radio and Electricity (part scores from C-40-A), Electricity (one of the mechanical information tests—devised by the Air Corps), and General Electrical Information, a Signal Corps test.



administered in 90 minutes. Of this time, 40 minutes is devoted to the General Classification Test, which is given to all men entering the Army. The other three tests, require only 30 minutes actual testing time.

#### SUMMARY

The United States Army requires, in the building up of the armed forces, many radiotelegraph operators. The number needed far exceeds the available supply and has necessitated setting up a selection program to identify those men with aptitude for radio telegraphy. Training consists of both code and non-code material, which appear to involve different aptitudes. Inability to develop acceptable speed in code is by far the most common reason for failure in radio operator courses. The second most common reason for failure is a low grade in radio mechanics course.

All the data on the problem have been collected in various installations throughout the country and sent to the Personnel Research Section for analysis. Collection of data has been complicated by such factors as the difficulty of securing adequate validity criteria, generalizing results from one branch of the Army to another, and securing control of conditions under which data are collected. The selection of men for training on the basis of scores on the Army General Classification Test and the Radiotelegraph Operator Aptitude Test-1, X-1 that is generally followed has tended to reduce the value of the validity coefficients that are secured and make the tests appear less valid than is probably the case.

*Code Speed and Code Tests.* The two tests that have been most used in selection are the Radiotelegraph Operator Aptitude Test (ROA-1, X-1), an auditory discrimination test, and various forms of the Code Learning Test, a work-sample test. On the basis of data which have been collected, the Code Learning Test is the better of the two instruments. It is highly reliable (in the high .90's) and possesses considerable validity in terms of the development of code speed, the validity coefficients being generally in the .40's. The ROA-1, X-1 Test is only moderately reliable (in the .70's) and is less effective as a predictive instrument, the validity coefficients with code speed being generally in the .30's. ROA-1, X-1, shows definitely improved reliability and validity when administered twice, and this procedure has been made standard.

Both of the tests will yield rather high validities for groups

which are heterogeneous as to previous code experience, since experienced men do better both on the tests and in the learning situation. The tests perform less well for men without any previous experience in code. Correlations between the two tests have varied markedly in different groups but appear to cluster in the low .40's.

*Code Speed and Other Measures.* There seems to be little relation between years of education and ability to attain code speed. Initial interest in receiving training appears to have no effect on ability to score on the Code Learning Test-1, X-1, and little effect on the number of hours needed to attain various code speeds. Musical instrument experience is related slightly to ability to develop code speed. The relationship, however, is too low to justify giving it special consideration in selection. Since men with musical instrument experience tend to make higher scores on the CLT-1, X-1, they have a greater chance of being selected for training than those without such experience. Scores on the Seashore Test of Musical Talent show some relation to code learning, but other measures have been found to be better adapted to the problem.

Speed in code apparently bears a low but positive relationship to mental ability as measured by the Army General Classification Test. The groups in which this relationship was studied were composed of men drawn from the upper end of the grade scale on mental ability tests which would tend to reduce the value of the resulting coefficient. The relationship between speed in code and tests on electricity, mathematics, mechanical information, physics and the like, is low, but generally positive.

*Non-Code Material and Various Measures.* Validity criteria for the non-code materials were grades in courses dealing with the non-code aspects of radiotelegraph operator training. Correlations between scores on ROA-1, X-1 and scores on non-code courses were all positive but low. Scores between the Army General Classification Test and non-code course grades were all higher than that with code speed, ranging from .23 to .50. Eleven tests were found which related rather highly to grades in non-code courses, and which intercorrelated among themselves moderately well. Four were selected which gave a multiple correlation coefficient of .75 with average grade in five electrical courses.

## PSYCHOLOGISTS IN GOVERNMENT SERVICE<sup>1</sup>

BY DAEL WOLFLE

*Emergency Committee in Psychology, National Research Council*

- ABT, ENSIGN LAWRENCE EDWIN, D-V(G), USNR.  
ACKERSON, 1ST. LT. LUTON, Army of the United States.  
\*ARTHUR, MARY GRACE, 1044 Lowry Medical Arts Building, St. Paul, Minn., Consultant, Indian Service.  
BARTLETT, NEIL R., Box 25, U.S. S/M Base, New London, Conn., Research.  
BARTLEY, S. HOWARD, Dartmouth Eye Institute, Dartmouth Medical School, Hanover, N. H., Research.  
BELL, 1ST. LT. HUGH M., Army of the United States.  
BOLTON, ROBERT L., JR., Social Security Board, Region IV, Washington, D. C., Occupational Analyst.  
BRIDGES, CLAUDE F., Occupational Standards Unit, Air Service Command, Army Air Forces, War Department, Fairfield, Ohio, Personnel Psychologist.  
CARTWRIGHT, DORWIN, Department of Agriculture, Division of Program Surveys, Washington, D. C., Associate Social Science Analyst.  
CHAPANIS, ALPHONSE, Aero-Medical Research Laboratory, Wright Field, Dayton, Ohio, Assistant Psychologist.  
CHASE, 2ND LT. WILTON P., Army of the United States.  
CHRISTENSON, PFC. JAMES A., Army Air Forces.  
CLEVELAND, EARLE A., Classification Division, Hq., A.A.F.T.T.C., Knollwood Field, N. C., Personnel Technician.  
CROFT, MAJOR LYSLE W., Army of the United States.  
EISENBERG, PHILIP, U. S. Employment Service, 1835 North 3rd Street, Harrisburg, Pa., Occupational Analyst.  
ELLSON, DOUGLAS G., Division of Program Surveys, Bureau of Agricultural Economics, Department of Agriculture, Washington, D. C., Assistant Social Science Analyst.  
EVANS, J. E., Department of Psychology, Iowa State College, Ames, Ia., E.S.M.W.T. (Psychol. in Mgt.); Research, N.R.C., Civilian Aircraft Pilots.  
\*GESELL, ARNOLD, 14 Davenport Avenue, New Haven, Conn., Member Advisory Committee on Day Care of Children of Working Mothers (Children's Bureau).  
HELD, LT. OMAR C., USNR.  
\*HENRY, FRANKLIN M., University of California, Berkeley, Cal., Research.  
HEWSON, LOUISE R., Neurological Institute, New York, N. Y., Research Psychologist, N.R.C., Study of Head Injuries.  
HOROWITZ, EUGENE L., Area Study Section, Bureau of Intelligence, Office of War Information, Washington, D. C., Analyst.

<sup>1</sup> This list supplements those published in the *Psychological Bulletin*, 1942, 39, 385-403, and 631-633.

\* Indicates part-time.

- HUNT, LT. WILLIAM A., USNR (erroneously reported as 1st Lt., AUS, this Journal, June, 1942, page 395).
- JANUS, SIDNEY Q., Social Security Board, Atlanta, Ga., Regional Occupational Analyst.
- JENNESS, 1ST LT. ARTHUR FREEMAN, Army of the United States.
- KENDALL, ENSIGN WILLIAM, USNR.
- KLEIN, GEORGE STUART, Psychological Research Unit No. 1, Nashville Army Air Center (A.A.F.C.C.), Nashville, Tenn., Psychological Assistant.
- KROUT, MAURICE H., 823 Dobson, Evanston, Ill., Psychological Examiner, Milwaukee Induction Station, Milwaukee, Wis.
- KURTZ, ALBERT K., Psychological Corporation, 522 Fifth Avenue, New York, N. Y., Project Director of a Defense Research Project.
- LANGMUIR, CHARLES R., 437 West 59th Street, New York, N. Y., Technical Assistant to N.R.C. Committee on Service Personnel, Selection and Training.
- LEHNER, LT. GEORGE F. J., Army Air Forces.
- LODGE, LT. (jg) GEORGE T., USNR.
- \*LOFGREN, P. V., 1060 Peralta Avenue, Berkeley, Cal., Psychometrist, E.S.M.W.T. Program.
- \*MACHT, MARTIN B., Department of Physiology, Johns Hopkins Medical School, Baltimore, Md., Psychological Examiner.
- MADDEN, ENSIGN WILLIAM F., H-V(S), USNR.
- MCCOLLOM, 1ST LT. IVAN N., Army Specialist Corps.
- \*MCLEOD, L. S., University of Tulsa, Tulsa, Okla., Interviewing and Testing, Tulsa Army Induction Station; Chief Examiner for C.A.A. for C.P.T. Applicants.
- MENEFEE, AUDREY GRANNEBERG, Foreign Broadcast Monitoring Service, F.C.C., Washington, D. C., Broadcast Analyst.
- MILLER, JOSEPH, 83 North Washington Street, Wilkes-Barre, Pa., Testing, Wilkes-Barre Induction Center.
- MILLER, CAPTAIN NEAL E., Army Air Forces.
- MILLER, LT. VERNON L., H-V(S), USNR.
- MITCHELL, LT. (jg) MILDRED B., W-V(P), USNR.
- \*MURRAY, M. E. MIRIAM, Child Welfare Division, Public Welfare Board, Bismarck, N. D., Chief Examiner, N.R.C. Civilian Pilot Training Program.
- NEUBERG, MAURICE J., St. Taris Park, R.D. No. 2, Springfield, Ohio, Civilian Administrator, Engineering Maintenance Officers Training School, Tateson Field.
- NEWLAND, LT. T. ERNEST, USNR.
- NEWMAN, SIDNEY H., 7007 Fordham Court, College Park, Md., Communications Analyst, Office of War Information, Washington, D. C.
- \*OLOM, CHARLES L., Southwestern Louisiana Institute, Lafayette, La., Chief Examiner for C.P.T. Applicants of C.A.A.
- \*PAYNTER, RICHARD H., Long Island University, Brooklyn, N. Y., Testing Applicants for the Civilian Pilot Training Course.
- PERIN, C. THEODORE, 430 West South Street, Hillsboro, Ohio, Personnelist, Occupational Standards Unit, Civilian Personnel Branch, Air Service Command, U. S. Army Air Forces.
- \*RABIN, ALBERT I., New Hampshire State Hospital, Concord, N. H., Chief Examiner, C.A.A. Testing Program.

RAINES, 1ST LT. LESTER, Army of the United States.

\*ROSEN, ESTHER KATZ, 239 West Allen Lane, Philadelphia, Pa., Interviewing Applicants for Women's Army Auxiliary Corps.

ROTHNEY, 1ST LT. JOHN W. M., Army Air Forces.

RUSMORE, ENSIGN JAY T., USNR.

\*SCOTT, WALTER DILL, 1729 Chicago Avenue, Evanston, Ill., Chairman, Solid Fuels Advisory War Council.

\*SEITZ, CLIFFORD P., College of the City of New York, New York, N. Y., Research.

SOLOMON, RICHARD L., Department of Psychology, Brown University, Providence, R. I., Research.

SPEER, GEORGE SCOTT, Recruiting and Induction Station, Fort Sheridan, Ill., Psychologist.

STEIN, LT. SEYMOUR P., Army Air Forces.

STOGDILL, LT. (jg) RALPH M., U. S. Maritime Service.

SWITZER, CAPTAIN ST. CLAIR A., Army Air Forces.

\*TERRY, PAUL W., College of Education, University of Alabama, University, Ala., Chief Examiner, C.P.T. Testing Program.

\*THORNDIKE, E. L., Teachers College, Columbia University, New York, N. Y., Committee of State Department on Examinations for Candidates for Foreign Service.

TYSON, 1ST LT. ROBERT, Army Specialist Corps.

\*VITELES, MORRIS S., 106 College Hall, University of Pennsylvania, Philadelphia, Pa., Chairman, N.R.C. Committee on Selection and Training of Aircraft Pilots.

\*WAGONER, LOVISA C., Mills College, Oakland, Cal., Consultant for Nursery Schools, War Recreation Authority.

\*WARDEN, CARL JOHN, Department of Psychology, Columbia University, New York, N. Y., Director of Research Project.

WARNER, LUCIEN, Special Services Division, Office of War Information, Washington, D. C.

WARREN, CAPTAIN NEIL D., Army Air Forces.

WOODRING, 1ST LT. PAUL, Army Specialist Corps.

WRENN, LT. C. GILBERT, USNR.

YOUTZ, 2ND LT. RICHARD PARDEE, Army Air Forces.



## PSYCHOLOGISTS IN THE NAVY

BY C. M. LOUTTIT

*Lieutenant Commander, U.S.N.R.*

In October, 1940, when the writer first reported for active duty at the United States Naval Medical School, he was the only psychologist on active duty in the Navy. At that time plans were being made to use a limited number of psychologists as members of psychiatric examining units at Naval Training Stations. Beyond this program there were no systematic plans for utilizing psychologists.

Today (February, 1943) the situation is quite different. As will be seen in the accompanying table, over one hundred and thirty psychologists, including women in the WAVES, are being used in several different programs. The numbers indicated can be taken only as approximate because of almost constantly changing conditions. Very probably there are some additional psychologists in the Navy who are not engaged in psychological work, but of these we have no record. In at least one case a psychologist, Ensign James S. Maddox, in command of an Armed Guard crew, has been reported as missing in action.

### PSYCHOLOGIST OFFICERS IN THE U. S. NAVAL RESERVE BUREAU OF NAVAL PERSONNEL:

#### *Washington:*

##### Training Division:

Standards and Curriculum Section—7 men, 2 women.  
Quality Control and Liaison Section—2 men.

#### *Field:*

##### Training Stations:

Selection Office—18 men, 2 women.  
Operating Bases (personnel work)—6 men.  
Schools (teaching)—4 men, 1 woman.

### BUREAU OF MEDICINE AND SURGERY:

#### *Washington:*

##### Aviation Medicine Research Division:

Aviation Psychology Section—6 men.

*Field:*

(Psychiatric Examining Units), Naval Training Stations and Marine  
Corps Barracks—30 men, 1 woman.  
Aviation Cadet Selection Boards—25 men.  
Pre-Flight Schools—4 men.  
Naval Reserve Aviation Bases—17 men.  
Naval Air Stations—8 men.

## BUREAU OF SHIPS:

*Washington:*

Officer Personnel Office—2 women.

## OFFICE OF NAVAL OPERATIONS:

*Washington:*

Director of Naval Communications—7 men.

## WOMEN PSYCHOLOGISTS IN THE WAVES, SPARS, AND MARINE CORPS (W-R)

BY MARJORIE K. BREMNER  
*Ensign, United States Naval Reserve*

The Women's Reserve of the United States Navy numbers among its officers many who have been trained in the field of psychology. These officers are working in various Bureaus or at W-R (Women's Reserve) training schools or training stations, many of them in the field of selection or personnel. Although their work at the training schools and stations has been exclusively with the WAVES program, they have also been working at the Bureaus with the selection and personnel program for the men.

Two psychologists are currently assigned to the Bureau of Ships, in Washington, D. C. Lt. (jg) Helena Elizabeth O'Neill, USNR, is working in the field of selection and assignment of engineering officers; and Ensign Lillian Johnson, USNR, is assistant personnel officer of the Radio Division, handling problems of officer and civilian personnel, and administrative duties. Before joining the WAVES, Lt. (jg) O'Neill was research specialist at the Catholic University School of Nursing Education; Ensign Johnson was assistant professor in the Department of Psychology, West Kentucky Teachers' College.

In the Trainee Requirements Unit, Standards and Curriculum Section, Training Division, at the Bureau of Naval Personnel, in Washington, are Lt. (jg) Gwendolen Schneider, USNR, formerly vocational counsellor at the University Testing Bureau, University of Minnesota; and Ensign Marjorie K. Bremner, USNR, formerly staff psychologist at the Bureau of Child Study, Chicago Board of Education. The function of the Trainee Requirements Unit is to achieve the efficient assignment of naval enlisted personnel to schools and duties in the Navy.

Lt. (jg) Helen Campbell, USNR, formerly instructor of psychology and education at Swarthmore College in Pennsylvania, is an instruction officer, W-R, at the Naval Reserve Midshipman's School, Mount Holyoke College, South Hadley, Massachusetts. In the selection office at the Naval Training School, (W-R) The

Bronx, New York, (for enlisted women), is Lt. (jg) Eva Bond, USNR; she was previously professor of psychology, Richmond Division, College of William and Mary, Richmond, Virginia. Lt. (jg) Mildred Mitchell, formerly psychologist for the State of Minnesota in the Bureau of Mental Health, is serving as a clinical psychologist at the Naval Medical Center, Bethesda, Maryland.

Ensign Helen May, USNR, who has had experience in clinical psychology and testing, was formerly assigned to the Selection Office at the Naval Training Station (W-R), Iowa State Teachers' College, Cedar Falls, Iowa, but has recently been transferred to the Naval Training School (W-R) at the Bronx, New York City. Ensign Almira Blanche Davis, formerly assistant professor of child psychology at the University of Tennessee, is assigned to the Naval Training Station (W-R) at Cedar Falls, Iowa.

The Director of the SPARS (Women's Reserve of the Coast Guard), Lt. Comdr. Dorothy Stratton, was formerly Dean of Women and professor of psychology at Purdue University. The newly formed Women's Reserve of the United States Marine Corps has assigned to the classification section of the personnel division Captain Cornelia Taylor Williams, USMC, W-R, formerly instructor and research counselor at the General College, University of Minnesota.

This brief summary is probably not complete; and it may be expected that other psychologists will be among those women who later join the services. In addition to the officers listed above, a fairly large number of women who have had personnel experience are now working in that field in the Naval Reserve.

## THE INTERSOCIETY CONSTITUTIONAL CONVENTION OF PSYCHOLOGISTS

The national organizations which were invited by the Emergency Committee in Psychology to send representatives to the I.C.C. have chosen the following as delegates and alternates:

*American Psychological Association*, 5 delegates and 5 alternates named. *Delegates*: John E. Anderson, Leonard Carmichael, John F. Dashiell, Calvin P. Stone, Robert M. Yerkes. *Alternates*: Clark L. Hull, Steuart H. Britt, Ernest R. Hilgard, Herbert Woodrow, Willard C. Olson.

*American Association for Applied Psychology*, 5 delegates and 5 alternates named. *Delegates*: Paul S. Achilles, Steuart H. Britt, Alice I. Bryan, Edgar A. Doll, C. M. Louttit. *Alternates*: Sidney L. Pressey, Arthur W. Kornhauser, Carl R. Rogers, Robert A. Brotemarkle, William C. Trow.

*Society for the Psychological Study of Social Issues*, 3 delegates and 3 alternates named. *Delegates*: Gordon W. Allport, Gardner Murphy, Theodore Newcomb. *Alternates*: Ernest R. Hilgard, Otto Klineberg, Goodwin Watson.

*Society of Experimental Psychologists*, 2 delegates and 3 alternates named. *Delegates*: Edwin G. Boring, Robert S. Woodworth. *Alternates*: Walter S. Hunter, Samuel W. Fernberger, Donald G. Marquis.

*Psychometric Society*, 5 delegates and 2 alternates named. *Delegates*: J. W. Dunlap, Harold A. Edgerton, Paul A. Horst, Irving Lorge, M. W. Richardson. *Alternates*: P. J. Rulon, Ben D. Wood.

*National Institute of Psychology*, 1 delegate and 2 alternates named. *Delegate*: Ernest R. Hilgard. *Alternates*: G. R. Wendt, A. T. Poffenberger.

*National Council of Women Psychologists*, 2 delegates and 2 alternates named. *Delegates*: Florence L. Goodenough, Gladys C. Schwesinger. *Alternates*: Theodora M. Abel, Marion A. Bills.

*Department of Psychology of the American Teachers Association*, 1 delegate named. *Delegate*: Herman G. Canady.

*Section I, American Association for the Advancement of Science*, 2 delegates named. *Delegates*: H. E. Garrett, Edna Heidbreder.

The Convention will be held in New York City May 29-31, 1943. Preliminary arrangements have been entrusted by the Emergency Committee in Psychology to its Subcommittee on Survey and Planning.



## PSYCHOLOGY AND THE WAR: NOTES

*Army Specialized Training Program.* In memorandum No. W 150-1-43 of the Adjutant General's Office of January 27, 1943, on the subject "Call to Active Duty of Students Enlisted in the Enlisted Reserve Corps, Unassigned Group," the following fields of training are listed as accepted under the Army Specialized Training Program as "approved technical engineering courses":

- |  |                                       |
|--|---------------------------------------|
| 1. Aeronautical engineers  | 7. Mechanical engineers               |
| 2. Automotive engineers  | 8. Radio engineers                    |
| 3. Chemical engineers  | 9. Chemists                           |
| 4. Civil engineers   | 10. Mathematicians                    |
| 5. Electrical engineers  | 11. Meteorologists                    |
| 6. Heating, ventilating, refrigerating, and air-conditioning engineers | 12. Physicists, including astronomers |
|  | 13. Psychologists                     |

*Revision of Selective Service Occupational Bulletin Nos. 10 and 11.* In the original Selective Service Bulletin No. 10 issued June 18, 1942, psychology was listed as one of the "critical occupations" (see *Psychological Bulletin* 1942, 39, 522-528). Psychology was continued in the list in the revision of Occupational Bulletin No. 10 issued on December 14, 1942 (see *Psychological Bulletin*, 1943, 40, 219-221). On March 1, 1943, however, the list of critical occupations in Occupational Bulletin No. 10 was amended by adding the field of petroleum engineering and dropping the fields of industrial engineering and psychology. In Occupational Bulletin No. 11, amended March 1, 1943, a policy of deferment is outlined with reference to students in the scientific and specialized fields listed in Occupational Bulletin No. 10.

## BOOK REVIEWS

TOLMAN, EDWARD C. *Drives toward war*. D. Appleton-Century, 1942. Pp. xv+118.

In a charmingly apologetic and disarming Foreword Professor Tolman tells us that though others may value and exalt war he finds it horrible and cruel. As a preliminary then to preventing its recurrence he seeks to analyze the nature of war and to discover its psychological reasons and causes. His procedure, he admits, is over simple and bizarre, and only justified by a neurotic disgust with war. To use his own words: "Knowing but little sociology, economics, history, or political science, I, as a mere psychologist, will adapt concepts derived from the behavior of rats and chimpanzees, combine them with certain Freudian notions, and then attempt to apply the result to the most central and the most grievous problems of human society" (xii).

The "psychological" causes of war he finds in the frustration of those convenient little "biological" drives which make trouble when interfered with and allow peace and harmony when they have full sway. Most important are these drives, for they provide the ultimate and basic needs. "Man's houses and heating systems, his clothes, airplanes, governments, crimes, religions, and bombs are, we must assume, to be ultimately explained as but complicated outgrowths of those same biological drives and social techniques" (53). Social techniques are also drives (self-abasement, self-assertion, collective drives) which are ancillary to the basic biological drives. The drives originally are determined by heredity, but organisms learn to direct them, or they become modified and converted by repression, fixation, projection, etc.

Now the big plan to abolish war and other most central and most grievous problems is so frustrate the frustration of the drives. It is to bring about the "psychologically adjusted man." Three "practical" devices are proposed. (1) Evolve an economic order to abolish biological frustrations. (2) Invent an educational and social system to make easy the identification of children with parents. (3) Create a supranational state.

Although the Foreword forefends criticism, the text still stimulates certain questions. Even if one is willing to accept the drive-instinct conception is there any connection between these little demons and the complex phenomena of social existence and international relations? Is it possible to substitute rat and chimpanzee behavior for politics, economics, and social history? Should even such a small book be totally dissociated from societal and intersocietal institutions? Is psychology too poor to do them reverence? On the contrary, is not the psychological essence of war comprised of such complex institutional activities as are integrated with type and variety of citizenship, professional occupation, and family up-

bringing? Indeed, in his Foreword the author hints that such institutional factors lie at the root of his own hatred of war. Again, if one takes instincts seriously why discriminate against the pugnacity drive? Is not such discrimination evidence that drives are arbitrarily chosen to make a case? But if one does take instincts seriously, is it not a *tour de force* to attempt to interfere with drives or to bewail their results? And finally, in a world of drives what difference does war or peace make?

J. R. KANTOR.

*Indiana University.*

BORNSTEIN, J. & MILTON, P. Action against the enemy's mind. Indianapolis: Bobbs Merrill, 1942. Pp. xxi+294.

The title of this book suggests a program of psychological warfare directed against the Axis Powers whereas only the last twenty pages touch lightly upon such a theme. Book One, written by Bornstein, describes German psychological warfare, its organization, principles, and methods, as it functioned in Europe and as it is applied in America. Close parallels are drawn between factional groups in all countries which provide a fertile soil for the germination of ideas likely to foster defeatism and an Axis victory. Especially selected for discussion are the problems of isolationism, Communism, anti-Semitism, and the fifth column. Only the consideration of German Americanism has a local setting. Book Two, written by Milton, consists of eleven short chapters which attempt primarily to portray conditions in America favoring the success of Axis propaganda. Again there is emphasis upon isolationism, the fear of Communism, and anti-Semitism but problems issuing out of conflicts between labor and capital, native and foreign born citizens, white and Negro populations, and among members of the United Nations are discussed along with possible solutions.

Few readers will fail to appreciate the service intended by the authors, that of presenting psychological warfare as a method of utilizing conflicting ambitions, hatreds, and ideas for the defeat of those who hold them. The plan of the book is sound in so far as it relates propaganda to dynamic ideologies. We may question, however, whether such an ambitious undertaking can succeed within the space of a brief volume. Excellent though the first part of the book is, brevity denies adequate treatment of German psychological warfare. When problems of the magnitude of those referred to in the second part of the book are presented as a succession of generalizations, no matter whether or not the generalizations are true, the reader will not be enlightened but confirmed in his existing allegiances and beliefs.

CHARLES BIRD.

*University of Minnesota.*

## NOTES AND NEWS

EDWIN B. TWITMYER, professor of psychology, chairman of the department of psychology and director of the psychological laboratory and clinic at the University of Pennsylvania, died on March 3, after a very brief illness. Professor Twitmyer was in his 69th year at the time of his death.

MARIA J. A. VAN DER LUGT has been appointed assistant professor of psychology, University of Vermont, to succeed R. M. COLLIER, who is a first lieutenant in the Army.

FREDERICK PISTOR has been appointed professor of education and psychology, Georgia State Woman's College, Valdosta.

F. G. MACOMBER, professor of education, University of Oregon, has been transferred to the WAAC training camp, Daytona Beach (Fla.) where part of his work deals with the psychological testing, classification, and assignment of incoming WAACs.

LOREN E. MESSENGER, assistant professor of psychology, Southern Oregon College of Education (Ashland), has been commissioned a lieutenant (s.g.) in the Navy.

MAJOR KARL T. WAUGH, formerly personnel officer of the department of public administration, Pennsylvania, is administering the Student War Loans Fund of the War Manpower Commission, under the direction of the U. S. Office of Education.

A. S. CLAYTON has been appointed professor of philosophy and psychology, Talladega (Ala.) College.

THOMAS W. HARRELL, on leave from the department of psychology of the University of Illinois, on duty at the headquarters of the Army Air Forces, has been promoted to the rank of major. His assignment is in the Personnel Research Section, Office of the Director of Personnel.

HENRY C. PEIFFER, instructor in psychology, San Diego (Calif.) State College, has been appointed registrar of the college.

RICHARD S. UHRBROCK, head of the research department in the Industrial Relations Division of the Procter and Gamble Company, Cincinnati, has been appointed consultant in the training within industry program of the War Manpower Commission by Chairman Paul V. McNutt. He is developing a program for the selection of new supervisors in organizations holding war contracts.

Michael Reese Hospital, announces that DR. S. J. BECK will offer his usual course this year in the Rorschach test. Accent will be on those less serious mental disturbances in which success in treatment appears possible. The differentiating patterns of the test, in these patients, will be studied from full response records, and contrasted with those found in more serious conditions. The course will be in session two two-hour periods daily for five days, June 7-11, 1943, inclusive. Interested persons are invited to communicate with the Department of Neuropsychiatry, Michael Reese Hospital, Chicago.

*Midwestern Psychological Association.* By a vote of 123 to 6, The Midwestern Psychological Association suspended operations for the duration in adopting the following resolution, proposed by the Council:

WHEREAS, The professional societies of the country have been asked to abolish or severely restrict travel to meetings and conventions; and

WHEREAS, Many of the members of the Midwestern Psychological Association are engaged in military or civilian government service; and

WHEREAS, The professional and scientific interests of the members of the Midwestern Psychological Association are being cared for by the American Psychological Association and other national agencies; therefore be it RESOLVED, That:

1. No further meetings or elections of the Midwestern Psychological Association be held until the restrictions on meetings are lifted;
2. Present officers whose terms of office expire in 1943 be replaced by newly elected officers who will serve until the next election;
3. Present officers whose terms of office do not expire in 1943 will continue to serve until the next election;
4. No dues be collected for the year 1942-43 or any subsequent year until meetings and elections are resumed and that those members whose dues for the year 1942-43 are already paid shall be credited with payment for the first year after the resumption of meetings and elections;
5. The officers of the Midwestern Psychological Association be authorized to conduct any necessary emergency business of the Association and to reopen the normal functions of the Association as soon as conditions permit.

New officers, elected in accordance with paragraph 2 of this resolution, are DR. S. L. PRESSEY, the Ohio State University, *President*, and DR. M. A. TINKER, the University of Minnesota, *Member of Council*.

When war time restrictions on travel are removed, a meeting will be called by the present officers, and normal functioning of the Association will be resumed.

DAEL WOLFLE,  
*University of Chicago,*  
*Secretary-Treasurer.*



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